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THE LAMARCK MANUSCRIPT IN HARVARD

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LAMARCK manuscripts are exceedingly rare. For until the last score years Lamarck was ranked as a discredited author, and his writings were thrown aside. Even the autograph collector, to whose nets almost everything is a fish, has hardly taken the pains to preserve his bare signature. The Harvard manuscript, accordingly, is an important document, especially in these days of Lamarckian revival. It is holographic, antedating, therefore, 1818-20, the years when Lamarck's eyesight was lost. It forms together a series of essays and drafts of later work, all in all about ninety leaves, of which fifty have writing on both sides. They are brought together in a volume with marbled sides and morocco back with the legend, "Manuscrits de Lamarck," the binding dating 1830-40. As a frontispiece there is inserted the Langlumé lithograph of Alexis Noël's portrait of Lamarck (1823). Following this is a table of contents, probably in the hand of the early owner of the manuscript. It reads:

Manuscrits | de | J. B. P. A. de Lamarck | Membre de l'institut de
france, Professeur-adminis- | trateur du Museum d'histoire naturelle, ect.
contenant|.

1° Système de Gall.....	20	feuillets
2° Idée et Imagination	19	"
3° Apperçus analytique de connaissances humaines.	11	"

4° Questions Zoologiques.....	9	"
5° Histoire naturelle	3	
6° Planches préparées pour les figures des genres qui feront partie de la 2 ^e édition des animaux sans vertèbres.....	19	feuillets
Total	81	feuillets

From this it will be noted that the papers were collected before 1835, the year of the appearance of the second edi-



Jean Baptiste, Pierre-Antoine Monet de Lamarek.

tion of the "Animaux sans Vertèbres," for it is stated that the drawings *will form* part of the second edition, not that they did form part of it.

This manuscript was presented to Harvard University in 1896 by Professor Alexander Agassiz, who appears to have discovered it in Paris. Its earlier provenance is unknown. My attention was called to it by my friend,

questions zoologiques
dont la solution est de première importance.

1^{re} question : les animaux et les végétaux étant des corps vivans, ces 2 sortes de corps se confondent-ils par un point commun des deux qu'elles forment ; ou existe-t-il quelque caractère exclusif et tranché qui distingue nettement les premiers des seconds ?

2^e question : peut-on mettre en évidence, par la citation de faits décisifs, que tous les animaux connus jouissent du sentiment ; ou qu'il n'y a qu'une partie d'entre eux qui voient doués de cette faculté ?

3^e question : peut-on prouver par des faits pareillement décisifs, que tous les animaux connus possèdent la faculté d'avoir des idées et de former cette détermination par pré-méditation qui fait agir soltairement, et permet de varier les actions ; ou qu'il n'y a qu'une partie des animaux qui jouissent de cette faculté ?

4^e question : y a-t-il quelque faculté animale qui ne soit pas un phénomène d'organisation et qui soit indépendante de tout système d'organes quelconque ; ou toute faculté qui n'est pas commune à tous les animaux, ne dépend-elle pas d'un système particulier d'organes qui y donne lieu ?

5^e question : tous les animaux connus possèdent-ils la totalité des systèmes particuliers d'organes qui composent l'organisation très compliquée des animaux les plus parfaits ; ou, quoique ces systèmes d'organes soient effectifs à la vie dans les organes animaux qui les possèdent, la vie dans d'autres animaux ne peut-elle pas exister

Dr. C. R. Eastman, who was generously instrumental in placing it in my hands. To him, therefore, and to Professor Samuel Henshaw, curator of the Museum of Comparative Zoology, my thanks are due for the privilege of examining it.

In further detail, and in the matter of its published or unpublished parts:

The *Système de Gall* is largely medical: it deals with the brain, its anatomy, comparative anatomy, physiology, pathology—the last in some detail as in idiocy, cretanism, suicidal mania, hereditary insanity. I cannot find that it has been published.

The second essay, *Idée et Imagination*, has certainly been published. It bears the note in Lamarck's hand, "*Articles du diction*," and is signed by compositors. Dr. Eastman suggests that it occurs in *Nouv. Dict. Hist. Nat.* of Deterville, 1818, a work I have not been able to consult. The writing indicates an earlier date than the remaining leaves.

The third portion, *Apperçu analytique des connaissances humaines, avec des divisions et des reflexions tendant à montrer leur degré de Certitude, leurs Sources, leurs Branches principales*, is probably the outline of his extended work (362 pp.) on the same subject published in 1820: it is entirely in his own hand and probably dates not later than 1818 (the year in which his eyes failed him).

Of the fourth manuscript, *Questions Zoologiques*, the first section is substantially as follows:

"Zoological questions whose solution is of first importance.

"First Question.—Animals and plants being living bodies (*corps*), do these two kinds of organisms become confused (*se confondent*) at a common point of the series which they form; or does there exist some exclusive and trenchant character, which distinguishes sharply the first from the second?

"Second Question.—Can one show by the citation of decisive facts, that all animals known are endowed with sensation;¹ or that there are only certain of them which are endowed with this faculty?

¹ Lamarck uses the word "sentiment." From several contexts, however, one concludes that more than "sensation" is intended, and that "con-

"Third Question.—Can one prove by facts equally decisive that all animals known possess the faculty of having ideas and of determining them by premeditation,—a premeditation which is formed *voluntarily* and which permits the actions to be varied; or are there only certain animals which enjoy this faculty?

"Fourth Question.—Is there some faculty in animals which is not a phenomenon of organization and which is independent of all systems of organs whatever; or does not every faculty which is not common to all animals depend for its origin upon a particular system of organs.

"Fifth Question.—Do all animals known possess the totality of the particular systems of organs which make up the very complicated organization of the most perfect animals; or, however essential are these systems of organs to the life in the animals which possess them, can not life in other animals exist without them?

"Sixth Question.—Is there known a single organ which is essential to animal life (in general) whatever be its function in the particular organism of which it forms a part; or must we not assume that life, whether of plant or of animal, needs no particular organ whatever, to enable it to exist in certain organisms.

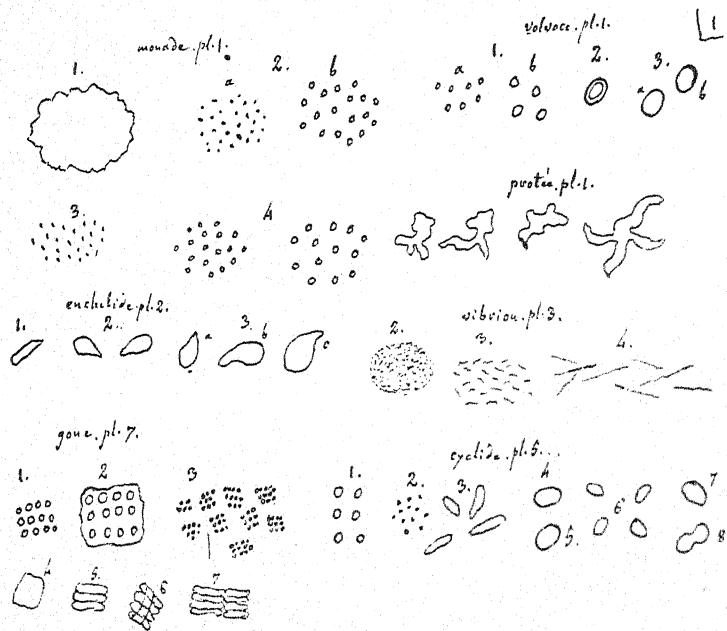
"Seventh Question.—Cannot *sensation* (*sentiment*) and *irritability* be regarded as one and the same organic phenomenon and can it not be proved by facts that every portion of an animal which is endowed with *irritability* is also endowed with *sensation*; or is not *irritability* with which all animals are endowed, whether in all their parts or in certain of them only, an independent phenomenon and distinct from the *sensation* enjoyed by many animals?

"Eighth Question.—Can it be established clearly that the facts of movement in the case of so-called *sensitive* plants demonstrate in these plants either *sensation* or *irritability*; or that these facts have no relationship whatever with those which demonstrate in animals on the one hand *sensation* and on the other *irritability*?

"Ninth Question.—The nerves alone are the organs of *sensation* since *sciosness* might often be the better rendering. Thus in the eighth question referring to sensitive plants he distinguishes sharply "*sentiment*" from "*irritabilité*." The latter gives the idea of unconscious reflex to stimulus, and the former then becomes antithetic, *i. e.*, conscious. The literal text in this question reads "peut on établir d'une manière évidente, que les faits de mouvement relative aux plantes dites *sensitives*, constatent dans ces plantes soit le *sentiment*, soit l'*irritabilité*; ou que ces faits n'ont aucun rapport avec ceux qui attestent les uns le *sentiment*, les autres l'*irritabilité* des animaux?" Again, contrasting questions second and third, it is clear that "*sentiment*" in the second question is distinguished from a higher form of consciousness, which is equivalent to reason (or intelligence). In general we assume that the phrase "*avoir le sentiment*" implies consciousness.

the faculty of sensation is lost in a part (of an animal) in which the nerves supplying it have been destroyed; now the question is whether every nerve produces a sensation when it is affected, and whether the nerves which bring the muscles in action as well as those which furnish the forces of action to the organs produce sensation, like the rest; or whether there are not particular and special nerves for the production of sensation, while the others function some for *muscular excitation*, the others only for putting different organs in a condition to execute their functions?

"Tenth Question.—Is there some constant and peculiar sign which will make us understand that a being differing from ourselves ex-



Lamarck's Pen-drawings of Microorganisms (MS. p. 145).

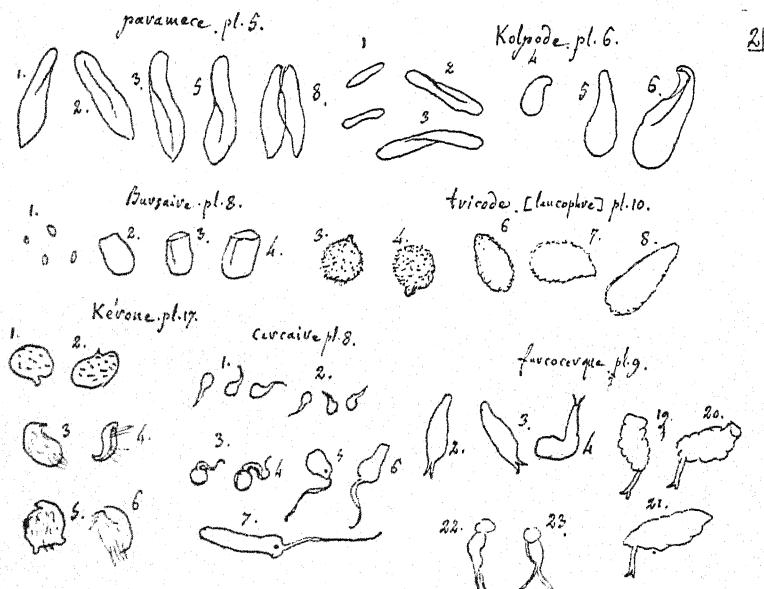
periences a sensation when it is stimulated (*affecté*), and can one always accept as a test the similar movement which it then executes; or, however in general an animal gives no other sign of a sensation produced than by the movements of its parts, can not these movements often deceive us and be due only to the *irritability* excited in the parts of the animal?

(I know no certain sign of a sensation produced save a *crisis* evoked by pain: but all animals are not able to give such a sign and those which have the power do not always use it.)

"SUPPLEMENTAL QUESTION:

"Eleventh Question.—If each particular system of organs gives rise to a particular faculty, can this faculty be found again in an animal in which the system of organs which produces it no longer exists; or can not this same faculty be regarded as destroyed when the system of organs which has given rise to it ceased to exist?"

These questions date from the period 1810-18, with the probability that they belong nearer the later than the earlier date, for in his "Philosophie Zoologique" (1809)

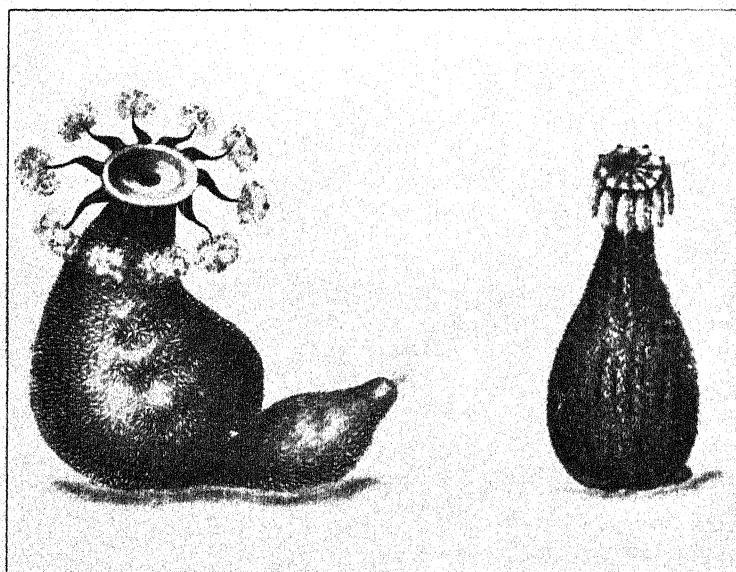


Lamarck's Pen-drawings of Microorganisms (MS. p. 147).

his views were by no means as advanced. He then spoke of the essential differences which distinguish plants and animals, and did not query their common origin, and did not seek a trenchant character which would serve to distinguish them. Moreover, he did not then query the possible kinship of sensation and irritability in sensitive plants and in animals, for at that time he had seen no reason to deny the elastic-fluid explanation for the sensitive movements of plants. Altogether these questions are of considerable interest in the study of the develop-

ment of Lamarck's views. They had, however, hardly reached the level of his Introduction to the second edition of his "Animaux sans Vertèbres" (1835). But we can regard them as sure steps in that direction, for similar ideas are here and there found in the Introduction. Indeed the second part of these *Questions Zoologiques*, MS. pp. 117-130, undoubtedly served as a first draft of this. Thus the present p. 117 is equivalent to p. 17 of the Introduction, and one can identify nearly all of the remaining leaves.

The fifth portion is headed "Histoire naturelle," and deals with its scope. It appears to have been a draft of a portion of the second edition of the "Animaux sans Vertèbres," for it is captioned "*Chap. 4. Connaissance des Corps organisés vivans que s'observent à la surface de notre globe et dans les eaux liquides,*" but these lines have been crossed out. The same ideas occur in the published work but in different form, so it is perhaps unnecessary to append here the entire section. The last page will give an idea of its tenor:



A Color-drawing of a Holothurian.

"Living bodies and inorganic bodies are the materials of natural history. They compose together the mass of the terrestrial globe, but they occur in very different proportions, for the first form a portion exceedingly small, while the second constitute almost the totality.

"Yet the bodies which are possessed of life are immumerable in the diversity of their species, and those, on the other hand, which lack it, exhibit in proportion only a small number. Indeed, we know hardly more than six or seven hundred species of minerals, while the number of species of living bodies can not be estimated as less than 100,000.²

"These considerations are not lacking in interest, provoking our reflections, and each one of them presents us a fact, an item of knowledge with which we have to reckon. In a word these singular bodies which possess life, which are so diversified, are yet the constituents of but a very small portion of the globe which man inhabits."

The final pages of the manuscript contain the following drawings:

Plate I, monads, volvoce, enchelide, protée, vibrion, goue, cyclida.

Plate II, paramèce, kolpode, Bursaire, tricode [leucophre], Kérone, cercaire, furcocerque.

Plate III, Ratule, tricocerque, vaginicole, folliculina, Brachion, furculaire.

Plate IV, urcéolaire, vorticelle, tubiculaire.

The remaining pages include the "animal of lepas balanus," with parts named, "millipora gelatinosa," a color sketch, a number of jelly fishes, including "dianée triedre," "orythie verte," "orthie hexaneme," "dianée proboscidale," "dianée dineme"; a number of nudibranchs, pteropods, and a beautifully colored drawing of a living holothurian.

²A generous estimate for that period—a number now several times exceeded within the insecta.

SYMBIOSIS IN FERN PROTHALLIA

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THE symbiotic associations so frequently met with in plants present one of the most interesting phenomena with which the biologist has to deal. While these associations are often not easily distinguishable from true parasitism, in many instances there is a genuine symbiotic relation and, although there may be a certain degree of parasitism, there is no question that these associations are for the most part beneficial to both of the forms concerned. Indeed, the very existence of one or both of the symbionts may depend upon it.

In most cases of symbiosis, one of the symbionts is a fungus, but this is not always so. Certain of the Schizophyceæ or blue green algae are very commonly associated with higher plants in what appears to be a symbiotic relation, although the nature of the association in this case is still very obscure. The Anthocerotaceæ and several of the liverworts, like Blasia, always have within their tissues colonies of a Nostoc, and the little water fern Azolla invariably harbors in each leaf a colony of the Nostoc-like *Anabaena Azollæ*. Nostoc has been found to occur in the roots of *Cycas revoluta* and Gunnera among the seed plants, and the Schizophyceæ also frequently constitute the "gonidia" of many lichens. The association of the nitrogen-fixing bacteria with the root nodules of the Leguminosæ is also a well-known case of symbiosis. Of the true algae there are a number of species recorded, e. g., *Chlorochytrium Lemnæ*, which live within the tissues of higher plants, but it is doubtful whether the host is in any way affected by the presence of the alga, which

presumably secures lodging, but not board, from its host.

The symbiotic association of fungi with green plants was first demonstrated in the lichens, but it is now known that many of the higher plants are regularly associated with fungi in what is undoubtedly a symbiotic relation. The best known cases of these are the mycorrhizal fungi connected with the roots of many trees, especially the Cupuliferae, and those which occur in the tissues of saprophytes growing in humus. These humus saprophytes are especially numerous among the Orchidaceae, e. g., *Neottia*, *Corallorrhiza*, *Cephalanthera*, etc., and in certain forms of the Ericales. The well-known Indian pipe, *Monotropa uniflora*, and the snow plant of the Sierra Nevada, *Sarcodes sanguinea*, are well known examples of these saprophytic Ericales. Many Orchidaceae and Ericaceae which possess chlorophyll are also to a greater or less extent saprophytic and show a well-developed mycorrhiza. In the case of chlorophyllless plants, there can be little doubt that the fungus enables them in some way to utilize carbonaceous compounds from humus. In the case of plants such as the trees referred to, where there is ample chlorophyll tissue, it is more likely that the rôle of the mycorrhiza is rather to supply nitrogen than carbon, and it is highly probable that in the case of chlorophyllless saprophytes as well, the fungus provides nitrogen. This has recently been demonstrated for the mycorrhiza found in the roots of various Ericaceae, e. g., species of *Erica*, *Vaccinium*, *Calluna* and *Oxycoccus*¹. In all of these it was shown that the fungus concerned, which seemed to be a species of *Phoma*, was able to assimilate free nitrogen in much the same way as is done by such nitrogen bacteria as *Azotobacter*.

A very complete study of the endophytic fungi of roots has been made by Janse.² He examined a very large

¹ Dr. Charlotte Ternetz. Über die Assimilation des atmosphärischen Stickstoffes durch Pilze. *Pringsheim's Jahrbücher*, XLIV, July, 1907.

² Les Endophytes Radieaux de quelques Plantes Javanaises. *Ann. du Jardin Botanique de Buitenzorg*, XIV, 54-201, 1895.

number of plants, mostly phanerogams, but also a number of liverworts and pteridophytes. His researches showed the presence of an endophytic mycorrhiza in a surprisingly large number of plants belonging to the most diverse families, from *Zoopsis*, one of the Hepaticæ, to *Vernonia*, a genus of Compositæ.

The study of the mycorrhizal fungi of the seed plants has called attention to the presence of similar fungi in the pteridophytes. The occurrence of a mycorrhiza in the roots of the Ophioglossaceæ was first shown by Russow.³ In 1884 Treub described a similar fungus from the gametophyte of *Lycopodium*. In his very important paper on the prothallia of *Lycopodium*⁴ he pointed out the universal occurrence of this fungus in *L. cernuum*, and in later papers he showed that this also occurred in *L. phlegmaria*, as well as in some other species, but it was apparently absent from the green prothallium of *L. salakense*. In 1895 I called attention to the presence of a similar endophytic fungus in the subterranean prothallium of *Botrychium virginianum*.

The past decade has been notable for the numerous important investigations upon the gametophytes of the Ophioglossaceæ and the Lycopodiaceæ and our knowledge of these is now quite complete, thanks to the labors of Bruchmann, Lang and Jeffrey. It is clear that in all prothallia of the subterranean, and hence purely saprophytic type, an endophytic fungus is invariably present. It has also been shown that a similar form occurs in the green prothallium of some species, at least, of *Lycopodium*; but so far as I am aware its occurrence in the green prothallium of ferns has not hitherto been recorded.

Some time ago, having occasion to look over slides of the prothallium and embryo of *Osmunda cinnamomea*, it was noted that many of the prothallia contained an endophytic fungus very similar to that found in *Botrychium*

³ Vergleichende Untersuchungen der Leibbündelkryptogamen. Mem. de l'Acad. Imp. des Sc. de Petersbourg, 1872, XIX, 107-118.

⁴ Etudes sur les Lycopodiacées. Ann. du Jardin Botanique de Buitenzorg, IV, 1884.

and Ophioglossum. This suggested the possibility of its occurrence in other green prothallia, and the forms which seemed to promise best were the Marattiaceæ, which in many ways seem to be the nearest relatives of the Ophioglossaceæ, in whose subterranean prothallia the endophyte regularly occurs. I therefore made an effort to obtain prothallia of the Marattiaceæ while collecting in Ceylon and Java, and procured prothallia of *Angiopteris erecta* Hoffm., *Kaulfussia aesculifolia* Bl., and *Marattia sambucina* Bl. The two former were carefully studied, and as was expected, the endophyte was found in nearly every case. The prothallia of *Marattia sambucina* were not examined, but the examination of a series of sections of *M. Douglasii* Baker, made some years ago, showed that in this species the endophyte was also present and presumably it occurs also in other species of Marattia.

The other family of ferns in which it was thought the endophyte might occur was the Gleicheniaceæ, a small family, mostly tropical and of wide distribution. The Gleicheniaceæ are considered to be related to the Osmundaceæ and it was thought that they also might show the presence of the endophyte. The prothallia have rarely been collected, but are not difficult to find if one looks for them carefully. Material of four species was secured, one being collected near Cape Town, the others in Ceylon and Java. In all cases the endophytic fungus was found in the older prothallia.

These investigations show conclusively that an endophytic fungus is normally present in the green prothallia of several Marattiaceæ, Osmundaceæ and Gleicheniaceæ, and it is highly probable that further research will show similar fungal endophytes occurring in the prothallia of many other ferns.

THE STRUCTURE OF THE ENDOPHYTE

Since the discovery of the endophytic fungus in the gametophyte of *Botrychium*, it has been found constantly

in all the investigated species of Ophioglossaceæ, and it is safe to assume that it is invariably present and is essential to the growth of the gametophyte.

The writer has recently had occasion to study the behavior of this endophyte in the gametophyte of several species of *Ophioglossum* and has described and figured this somewhat at length.⁵ The fungus consists of non-septate, large, branched hyphæ, which are strictly intra-cellular, passing from cell to cell through the cell walls, and they may often be traced for long distances. In all of the forms that have been investigated the fungus is confined to the older parts of the gametophyte, and never invades the meristematic tissues nor the tissues in the neighborhood of the young reproductive organs. There is in the cylindrical branches of the gametophyte of *Ophioglossum* a more or less definite infected zone inside the superficial tissues, while the central region remains almost entirely free from the endophyte. Sometimes fragments of mycelium are found upon the outside of the gametophyte, and these may occasionally be found to penetrate into the rhizoids and thus gain entrance to the inner tissues. The infection, however, probably in all cases takes place first while the gametophyte is still composed of very few cells. This was positively demonstrated in the germinating spores of *O. pendulum*, where only those young prothallia which succeeded in establishing a connection with the fungus developed beyond a three or four-celled stage. Otherwise they died after the nutrient matter in the spore was exhausted. Secondary infections, however, doubtless take place frequently. The form of the fungus growing outside of the prothallium is quite different from that within its tissues. The hyphæ in the former case are more slender and sometimes septa may be formed, while these seem to be quite absent from the endophytic hyphæ.

In material fixed with chromic acid, the structure of

⁵ Campbell. Studies on the Ophioglossaceæ. *Ann. du Jardin Botanique de Buitenzorg*, XXVI, 1907.

the hyphae is well shown. The walls, which in the ordinary hyphae are moderately thick, stain well with gentian violet, while in the finer granular cytoplasm there are more or less numerous small bodies which stain strongly with safranin and are with little question nuclei. Some of the cells of the host contain unmodified hyphae, which may be so numerous as to fill the cell cavity with a dense coil of filaments. In other cells the hyphae form masses of irregular swollen vesicles with much more delicate walls than the ordinary hyphae, and sometimes quite filling the cell. Besides the irregular vesicular swellings of the hyphae described above, there may occur large oval or round structures (Fig. 1) which resemble the young oogonia of *Pythium* or *Albugo*. These may have a diameter of nearly 50μ , but are usually smaller. The nuclei in these bodies are more numerous than in the vegetative hyphae, and finally may be very conspicuous (Fig. 1, C). This multiplication of the nuclei resembles the preliminaries of zoospore formation in the sporangia of *Saprolegnia* or *Pythium*, and occasionally there were seen free in the host cells small bodies that looked as if they might have been discharged from these large oogonium-like bodies. The latter are probably identical⁶ with

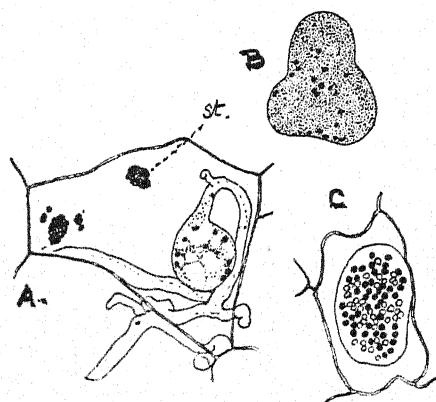


FIG. 1. A, Cell from the gametophyte of *Ophioglossum pectinatum*, showing the mycelium of the endophyte, and a young conidium (?); st, masses of disintegrating starch granules; B, large conidium (?) of the same; C, fully developed conidium (?), showing the numerous enlarged nuclei; all $\times 350$.

⁶ Jeffrey. The Gametophyte of *Botrychium Virginianum*. *Proc. Canad. Institute*, V, 1898.

the "conidia" described by Jeffrey in *Botrychium*, but do not show the thick walls of these conidia. Like these conidia of *Botrychium* they are not, usually at least, separated from the filament by a septum. The young cells of the gametophyte contain starch in the form of rather small and very distinct granules. As the endophyte invades these cells, the starch granules soon show evidences of disintegration, swelling up and losing their sharp contour and finally becoming aggregated in irregular masses of considerable size (Fig. 1, A, st). These finally are more or less completely digested by the fungus, but the nucleus of the host cell is in no way affected, and even where the cell is completely filled with the crowded hyphæ, the nucleus remains quite normal in appearance.

The endophyte of *Botrychium virginianum* (Fig. 2) closely resembles that of *Ophioglossum*, but is somewhat smaller in all its parts and occupies the whole central region of the massive gametophyte. The two sorts of

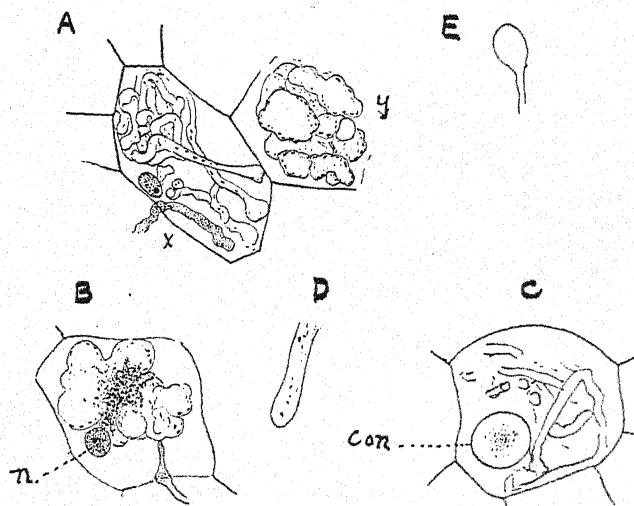


FIG. 2. A, two cells from the gametophyte of *Botrychium virginianum*, showing the two forms of the endophyte; B, a "digestive" cell, showing the degenerating varicose mycelium of the endophyte; n, the nucleus of the host cell; C, cell containing a conidium, con; D, fragment of one of the largest hyphæ; E, young conidium. All figures $\times 350$.

cells, *i. e.*, those with the filamentous hyphae (Fig. 2, *A*, *x*) and those containing the irregular vesicular mycelium, (*y*), are well differentiated, but are more or less irregularly mingled. The "conidia" (Fig. 2, *C*, *con*) are smaller and less numerous than in the endophyte of *Ophioglossum*, but have a much firmer membrane, as Jeffrey has described. These conidia were observed by Jeffrey to germinate by sending out a tube, and they are supposed to be special organs of propagation.

In a very important study of the endophytic mycorrhiza of the saprophytic orchid, *Neottia*, W. Magnus⁷ has shown that two types of mycelium exhibited by the endophytes are of very different nature. The slender cylindrical hyphae constitute the active portion of the fungus, which behaves like a parasite toward the cell which it invades, destroying the starch and probably other constituents of the cell, but not attacking the nucleus. The latter becomes much hypertrophied, a phenomenon that is not seen in the endophyte of the *Ophioglossaceae*. The swollen vesicular mycelium, however, is a degenerating structure and is itself destroyed by the cells of the host, which actually digest these fungus mycelia in much the same way that the cells of *Drosera* digest their prey. Some interesting similarities in the behavior of the contents of the digestive cells of *Drosera* and those of these humus saprophytes have been demonstrated. Figs. 2, *A* and *B*, show some of these cells in *Botrychium*; the varicose mycelium has very delicate walls, and in the older cells (Fig. 2, *B*) they seem to be disintegrating until finally the structure is completely destroyed and only a structureless lump is left. In *Neottia* this undigested mass is ejected into a central vacuole and becomes surrounded with a more or less evident cellulose membrane, separating it entirely from the protoplast after digestion is complete.

A comparison of the endophytes found in the green

⁷ Studien an der Endotrophen Mycorrhiza von *Neottia Nidus Avis L.* *Pringsh. Jahrb.*, XXXV, 1900.

prothallia of the various ferns referred to shows some differences which are probably not without significance. The structure of the mycelium and its general behavior are so much like those of the endophyte occurring in the strictly saprophytic gametophyte of the Ophioglossaceæ as to leave little doubt that the endophyte in each case

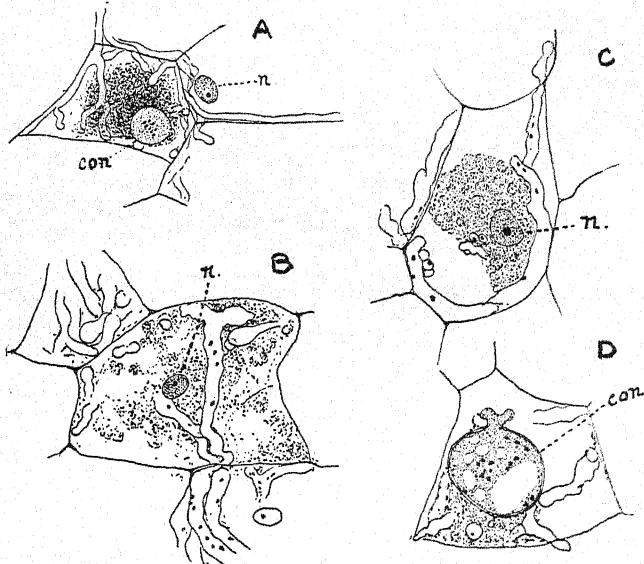


FIG. 3. Cells from the green gametophytes of several ferns, showing the character of the endophyte. All figures $\times 350$.

A, *Angiopteris evecta*; B, *Osmunda cinnamomea*; C, D, *Gleichenia pectinata*.

is the same, or at any rate closely related. The conidia (Fig. 3, A, C) are perhaps less frequent, but in form and structure are very like those of *Botrychium*. The most noticeable difference is the apparently complete absence of the "digestive" cells, *i. e.*, those that contain the varicose swollen mycelium. No indications were noted of the destruction of the fungus by the cells of the host and the former is evidently much more nearly a true parasite than is the case in the saprophytic gametophytes. In the infested cells of the green gametophyte the starch and chromatophores are destroyed evidently by the action of the endophyte, but the nucleus remains intact.

Of the ferns with green prothallia, the endophyte has been found, almost without exception, in the following: *Marattia Douglasii* Baker, *Kaulfussia esculifolia* Bl., *Angiopteris evecta* Hoffm., *Gleichenia* (*Eugleichenia*) *polypodioides* Sm., *G. (Mertensia) dichotoma* Willd. (= *G. linearis* (Burm.) Bedd), *G. (Mertensia) laevigata* Hooker, *G. (Mertensia) pectinata* Presl. In *Osmunda cinnamomea* it appears to be commonly but not always present, and in *O. Claytoniana* it could not be found. The number of slides of the last species examined was not very large and it is possible that further study of this species, as well as of *O. regalis*, will show its further occurrence in the Osmundaceæ.

Of the forms that were examined, that occurring in *Osmunda* and *Gleichenia* was the largest (Fig. 3, *B*, *C*) and equal in size to the endophyte of *Ophioglossum*. The form in *Angiopteris* was the smallest that was seen.

THE SIGNIFICANCE OF THE ENDOPHYTE

That the presence of the endophyte is necessary to the existence of all strictly saprophytic gametophytes is indicated by the failure of the germinating spores to develop unless they become associated with the fungus. Moreover, the universal occurrence of a similar endophyte in all humus-saprophytes among the seed plants indicates that in all of these chlorophylless plants the presence of the fungus is necessary for the existence of the host. Although it has not been directly proved, it is generally assumed that one rôle of the endophyte is the elaboration of some of the carbonaceous constituents of the humus. The infrequent communication between the external hyphae and the internal mycelium makes it unlikely that the nutritive products are directly absorbed by the fungus, and it seems much more probable that the rhizoids of the gametophyte are the direct agents of absorption. How the humus constituents are changed by the action of the fungus so that they are available for

the cells of the host is not clear and it is by no means impossible that some at least of the necessary carbon may be derived from the fungus itself in the digestive process to which it is subjected in the digestive cells. This seems plausible from the fact that in green prothallia, where presumably the plant is entirely able to supply its own carbon compounds through photosynthesis, these digestive cells appear to be wanting; or at any rate they were not observed in the several forms that I have studied. The experiments of Ternetz already referred to showing that certain fungi, including certain endophytic mycorhizæ, have the ability to assimilate free nitrogen, confirms the assumption of earlier authors that the fungus is useful to the host in supplying to it nitrogen compounds; but while this is probably a very important part of its functions, it seems to me that it is not perhaps the only one, and that the necessary carbon is also supplied directly or indirectly through the agency of the fungus.

As Magnus has very graphically shown, the relation of the two symbionts is mutually antagonistic, each one acting as a parasite on the other, but nevertheless the presence of the fungus is essential to the higher organism so long as the latter is destitute of chlorophyl; and the explanation of the wide-spread saprophytism exhibited by so many of the higher plants may be sought in this attempt to defend themselves against what was probably at first a strictly parasitic organism. Having acquired the power to attack and feed upon the parasite, the photosynthetic functions were more and more subordinated until a state of true parasitism (or saprophytism) resulted. The numerous semi-saprophytes like most of the green Ericaceæ and many green Orchidaceæ are good examples of transition stages, while the characteristic leafless humus saprophytes, such as the Monotropeæ and the chlorophylless Orchidaceæ, represent the fully developed phase of this peculiar form of symbiosis.

That this symbiotic association may occur in still lower organisms than the ferns is shown in the familiar case of the lichens, which are most perfect examples of this. It has been shown also that a similar association of fungus and host occurs in a good many liverworts. Cavers⁸ has studied this association with some care in the common liverwort *Fegatella*, as well as in other Hepaticæ. He found in *Fegatella* that the endophyte is beneficial to the growth of the host, the plant being more vigorous when the fungus was present. He assumed that this was due to the assistance given by the fungus in the assimilation of organic matter from humus or from other organic substrata.

This frequent occurrence of an endophyte in Hepaticæ makes the occurrence of this in the green prothallia of ferns quite readily understood. Whether in the latter it is an advantage to the host to have the endophyte present remains to be seen, but it is highly probable that such is the case. Once having acquired the habit of associating itself with the fungus, the gradual development of the purely saprophytic subterranean gametophytes of the Ophioglossaceæ from green forms similar to those of the Marattiaceæ, is readily conceivable. In the genus *Lycopodium* there is every degree from the strictly holophytic green prothallium of *L. salakense* to the subterranean chlorophylless gametophyte of *L. clavatum* or the still more specialized gametophyte of *L. phlegmaria*.

Presumably in the Ophioglossaceæ the evolution of the gametophyte has been very much the same as in *Lycopodium*.

⁸On Saprophytism and Mycorhiza in Hepaticæ. *New Phytologist*, II, 1903.

THE EVOLUTION OF THE TERTIARY MAMMALS,
AND THE IMPORTANCE OF THEIR
MIGRATIONS¹

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SECOND PAPER. OLIGOCENE EPOCH²

HAVING analyzed the *local evolution* and the *migrations* of the Eocene mammals (*Comptes rendus*, 6 novembre, 1905), I will now consider the corresponding data in regard to the Oligocene.

B. OLIGOCENE FAUNÆ.

I. Lower Oligocene (*Sannoisian or Lower Tongrian*). Two successive faunæ:

(a) Fauna of the *white marl* of Pantin, Romainville. The fauna of the *lignites* of Célas, Avéjan, Vermeil (Gard), of the limestone of Brunstatt and of Rixheim (Alsace) are probably not very distant from this. Without doubt the same is also true of several deposits in the South West of France: Fronsac and la Grave (Gironde), Sainte-Sabine, Duras, Issigeac, Saint-Cernin (Dordogne). A part of the phosphorites of Quercy,³ and of the "ter-
rain sidérolithique" of Fronstetten (Suabia) belong to the same level.

1. *Local Evolution*.—Continuance of the Palæotheriidæ (Palæotherium, Plagiolophus), of the Anoplotheridæ (last of Anoplotherium), of the Xiphodontidæ (last Xiphodon), of the Rodentia—Theridomyidæ (Theridomys).

¹ First paper, Eocene Epoch, in the February number of the NATURALIST.

² Extrait from the *Comptes rendus des séances de l'Académie des Sciences*, t. CXLII, p. 618 (séance du 12 Mars, 1906). Translated by Johanna Kroebel.

³ The remarkable fauna of the phosphorites is not a homogeneous assemblage, but a composite representing horizons from the Bartonian to the Stampian, inclusive. In general, therefore, I shall consider only those genera of the phosphorites that have been found elsewhere in the stratified deposits, and whose age can thus be positively determined.

2. *No new migration* is known.

This fauna seems to be simply a much-reduced remnant of the Ludian fauna and should be more properly included with the upper Eocene.

(b) Fauna of the *limestone of Brie*, of Hempstead (Isle of Wight), of Ronzon (Velay), of Lobsann (Alsace), of Calaf and Tarrega (Catalonia). A part of the phosphorites of Quercy and of the "terrain sidérolithique" (Bohnerz) of Veringendorf, Veringenstadt, of the Eselsberg, of the Hochberg and of Oerlingerthal near Ulm, belong to the same horizon. Possibly the beds of Monte-Promina (Dalmatia) belong to this horizon or to the preceding one.

1. *Local Evolution*.—Continuance of the Palæotheriidæ (Palæotherium, Plagiolophus), of Anthracotheriidæ (continuance of Brachyodus, and appearance of species of Ancodus, some species of *Anthracotherium*), end of the Anoplotheriidæ (last Diplobune), continuance of Cænotheriidæ (Amphimeryx, ?Cænotherium), of Canidæ (Cynodon, Cynodictis, Amphicynodon), of Erinaceidæ (Tetracus), of Theridomyidæ (Theridomys), of Hyænodontidæ (Hyænodon), of the Marsupial Didelphyidæ (Peratherium Amphiperatherium).

2. *Important North American migrations*: Sudden appearance of the Rhinocerotidæ (Ronzotherium), and of the Entelodontidæ (Entelodon).

3. Migrations of *unknown origin* of the Tragulidæ (Gelocus), of Mustelidæ (Proplesictis), of the Myomorph Rodentia (Cricetidæ), and perhaps of the Amphicyoninæ (beds of Tarrega).

II. Middle Oligocene (*Stampian or Upper Tongrian*), very numerous deposits: in the Paris basin, la Ferté-Aleps; in Germany, Ufhofen, Flonheim, Miesbach, lignites of Schluchtern, of Gusternheim and of Westerwald; in the basin of l'Allier, Bourroncle-Saint-Pierre, Bons, Perrier, Montaigut-le-Blanc, Champeix, Autrac, Saint-Germain-Lembron, Antoingt, Vodable, Solignat, Lamont-

gie, Nonette, *Orsonnette*, Malhat, Les Pradeaux, Les Chauffours, Bansat, Boudes, Chibrac, La Sauvetat, Jussat, Gergovia, Romagnat, Pérignat, Lemdes, Cournon, Mar-coin, Chaptuzat, Gannat, Saint-Menoux; in the basin of the Loire, Vaumas, Saint-Pourçain-sur-Bèbre, *Briennon*, Digoin; in the South East of France, Céreste, Manosque, clay of Saint-Henri near Marseilles, *les Milles* near Aix, Auzon near Alais; in the South West of France, Cestayrol, Saint-Sulpice, *Rabastens*, hill of *Saint-Martin*, *Montans*, *Salvagnac*, *l'Île d'Albi*, *Pont-Sainte-Marie*, Tournon, Capellier, Les Péries, *Villebramar*, la Milloque, Comberatière, Moissac, Beauville, Itier, Bourg de Visa, Montségur, etc.; in Switzerland, Blauen, La Conversion, near Lausanne; in Italy, Cadibona in Liguria, Monteviale and Zovencedo in Vicenza; in Austria, Trifail in Styria, and deposits in Dalmatia; lignites of Inca (Island of Majoreca); the larger part of the phosphorites.

It seems that from now on it will be possible to distinguish at least two horizons in this important stage: the lower (the principal deposits of which are given in italics in the preceding list), characterized by the persistence of the last representatives of *Palaeotherium*, of *Entelodon*, or of *Gelocus*; the upper by the abundance of large-sized *Anthracotherium* and *Aceratherium*, and the sudden appearance of the *Tapiridæ*.

For the stage as a whole, the facts in regard to evolution and migration are as follows:

1. *Local Evolution.*—Continuance of *Palaeotheriidae* (*last appearance*), of *Rhinocerotidae* (*Aceratherium*, *Diceratherium*), of *Chalicotheriidae* (*Schizotherium*), of *Anthracotheriidae* (*Brachyodus*, *Anthracotherium*, several phyla), of *Entelodontidae* (*last Entelodon*), of *Suidae* (*Propalaeochcerus*, *Palaeochcerus*), of *Cænotheriidae* (*Cænotherium*, *Plesiomeryx*), of *Tragulidae* (*last of Gelocus*, *Prodremotherium*, *Lophiomeryx*), of *Theridomyidae* (*Theridomys*, *Issiodoromys*, *Archæomys*), of *Cricetinidae* (*Cricetodon*), of *Talpidæ* (*Geotrypus*), of *Erinaceidae* (*Erinaceus*), of *Chiroptera* (*Palaeonycteris*), of *Creodontæ* (*last*

Hyænodon and last Pterodon, Dasyurodon), of Canidæ (Amphicyon), of Mustelidæ (Plesictis, Palæogale), of Viverridæ (Amphictis), of Marsupialia (Peratherium).

2. Migrations of *North American origin* of Tapiridæ (Protapirus, Paratapirus), and of Amynodontidæ (Cadureotherium⁴), and perhaps of the Felidæ-Machærodoninæ (Eusmilus).

3. Migration *probably from Africa* (and perhaps a little before the Stampian), of Edentata with normal vertebræ (Leptomanis and Archæorycteropus of the phosphorite beds).⁵

4. Migrations of *unknown origin* of Cervulinæ (Dremotherium, Amphitragulus), of Castoridæ (Steneofiber), of Myogalidæ (Echinogale, Myogale), of Tupaiidæ (Plesiosorex), of Soricidæ (Amphisorex, Sorex), of Lutrinæ (Potamotherium), of the Felidæ-Proælurinæ (Pseudælurus), and of Lagomorph Rodentia (Lagomyidæ, genus Titanomys).

III. Upper Oligocene (*Aquitanian*).

Principal deposits: in the Paris basin, Celles-sur-Cher; in the Bourbonnais, Saint-Gérand-le-Puy, Chaveroche; in Germany, Weissenau and Mombach near Mainz, Haslach, Eckingen near Ulm; in Switzerland the Gray Molasse of Lausanne, Othmarsingen, Hohe Rhonen; in Savoy, Pyrémont-Challonges; in Provence, Varages (Var); Boujac in the basin of Alais; in Catalonia, Rubi near Barcelona; in Bohemia, Tuchoritz; in Karinthia, Keutschach; in Hungary, Waitzen.

1. *Local Evolution.*—Continuance of Tapiridæ (Paratapirus), of Rhinocerotidæ (Aceratherium, Dicerather-

⁴ M. Boule (*Comptes rendus*, 18 mai, 1896) has endeavored to prove an affiliation between Cadureotherium and certain Ungulata of Patagonia, such as Astrapotherium; this relationship would be interesting, if demonstrated, for it would imply a *Patagonian migration* in the Oligocene period. But the supposed affinity rests, in my opinion, upon rather superficial resemblances of the dental system.

⁵ I do not believe in the existence of South American Edentata in the Oligocene of the phosphorite beds. The *Necrodasypus* of Filhol seems to me to be a dermal plate of a Reptile related to *Placosaurus* Gervais.

rium), of Chalicotheriidæ (Macrotherium), of Anthracotheriidæ (Brachyodus, last of Anthracotherium), of Suidæ (Palæochœrus, ? Doliochœrus), of Cænotheriidæ (Cænotherium, Plesiomeryx), of Cervulinæ (last Dremotherium and Amphitragulus), of Theridomyidæ (Theridomys), of Myoxidæ (Myoxus), of Eomyidæ (Rhodanomys), of Sciuridae (Sciurus), of Castoridæ (Steneofiber), of Lagomorph Rodentia (Titanomys), of Talpidæ (Talpa), of Soricidæ (Sorex), of Erinaceidæ (Palæoerinaceus, Erinaceus), of Canidæ (Amphicynodon, Cephalogale), of Amphicyoninæ (Amphicyon), of Mustelidæ (Stenogale, Plesictis, Palæogale), of Lutrinæ (Potamotherium), of Viverridæ (Amphictis, Herpestes), of Felidæ (Proælurus), of Marsupialia (the last European Didelphyidæ).

2. Smaller migrations of *unknown origin* of the Dimylidæ (Dimylus, Cordylodon).

The Aquitanian fauna is chiefly an impoverished residue of the Stampian.

Important migrations begin again with the Miocene epoch, and these will form the subject of a later paper.

OBSERVATIONS REGARDING THE CONSTANCY OF MUTANTS AND QUESTIONS REGARDING THE ORIGIN OF DISEASE RESISTANCE IN PLANTS¹

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It is not my purpose to develop a controversy as to theories, but, in pointing out some features of my studies upon disease resistance, it seems necessary to raise some question concerning the rapid development of the mutation theory which I believe to be worthy of close thought before we accept this theory as replacing, in entirety, the doctrine of evolution as formulated by Darwin.

The DeVriesian school has pointed out one way in which plants and animals originate new individuals with characteristics apparently new. The Mendelian formulas, especially as in late years developed, illustrate clearly how apparently new characteristics may appear to be caused to arise. This would seem to be a fair statement of all that has actually been accomplished.

I assent that most of the conceptions arising from the investigations of Mendel and DeVries are probably correct and, after considering expositions depending upon the experiments of many workers and having experimented sufficiently to understand the meaning of "mutants," "unit characters" and "elementary species," I recognize that these works added much light upon how evolution in plant life takes place, and that henceforth the conception of unit characters must largely form the theoretical working basis for practical breeding work. Yet I feel sure that Darwin's conception was sufficiently broad to embody both features as minor parts of the great concept of organic development. To accept De-

¹ Read before the American Breeders' Association, Washington, D. C., January 29, 1908.

Vries' doctrine of mutation as a substitution for the Darwinian conception is, I believe, to accept a part for the whole and to place before the farmer and stockman a doctrine which, if generally accepted as a substitute for the broad conception of Darwin, can not but be narrowing and injurious.

DeVriesian and Mendelian phraseology, in daily use, may be to blame, but in reading many of the late expositions one is led to question whether the doctrine of constancy of elementary species or constancy of unit characters can be accepted by biologists and breeders with any less damage to after progress than that which followed the once complacent acceptance of the Linnæan dogma of the constancy of species.

DeVries, of course, argues for the acceptance of the validity of evolution by mutations, and while one may readily concur that such mutations occur, one who works largely with cereal crops and has always recognized that the individual is the proper starting point for selection work, is apt to be astonished in reading his new work on "Plant Breeding," and falls to questioning whether after all, a mutation may not be merely a "fluctuating variation," big enough and stable enough to be recognized. Is it possible that the nature of these changes is different in kind or only in quantity and range of duration? Does accident play so large a part in plant development and plant breeding as indicated by most DeVriesian writers? One need not object because of the wonderful things said of elementary species, for good Darwinians have always believed in the existence of strains and subspecies which would admit of the name; nor should we expect any less of DeVries than that he should use the arguments related in "Nilson's Discovery" and those from such other experiments and expositions as would, when handled in certain lights, tend to show evolution by mutation. Yet, one who recognizes all of the possible merits that accrue from the conception of hypothetical unit characters and of the existence

of plant strains which admit of the designation "Elementary Species" may be pardoned if he is unable to accept another dogma of constancy, and is unable to subscribe to many DeVriesian conclusions regarding the comparative merits of mutation as opposed to adaptation and natural survival. Most of us, I believe, can only look upon a mutation as one of the types of variation through which the survival process brings about evolution. I believe that we still have to look for the underlying causes. An ardent Darwinian can well agree to the statement: "Species are derived from other species by means of sudden small changes which, in some instances, may scarcely be perceptible to the inexperienced eye,"² but may find points of doubt and refuse to follow to the limit when reading a number of statements found in the same work, for example, "From their first appearance they are uniform and constant."³ This statement refers to species. Again, "The conception of mutations agrees with the old view of the constancy of species. This theory assumes that a species has its birth, its lifetime, and its death, even as an individual, and that throughout its life it remains one and the same."⁴ "Each single type (be it species or subspecies or variety) is thus wholly constant from its first appearance and until the time it disappears either after or without the production of daughter species."⁵ This last sentence is certainly clear to the effect that a species is never changed, at least so as to affect its after progeny. It is equivalent to saying that all individuals after formation in the seed remain exactly like the parents and, taken with the other statements which insist that plants mutate, means that they remain constant until they change, which is an absurdity in argument. With this, we must assume that a man can recognize ultimate unit characters and can recognize just how many characters it takes to make a species. When

² DeVries. Plant Breeding. Page 9.

³ *Ibid.*, page 9.

⁴ *Ibid.*, page 8.

⁵ *Ibid.*, page 9.

we can do this, there will not be any use for the Breeders' Association. ". . . Mutations occur constantly, without preparation and without intermediates."⁶ I can not think of a mutation or change in species occurring without preparation and without intermediates. It is equivalent to saying without cause. Later, one reads, "We may confidently assume that each single mutation affects only a single unit."⁷ I would certainly agree with this statement if I could conclude from the author's other statements that he had in mind that a plant might be made up of an inconceivable number of unit characters, the ultimate nature of which may, after all, be only matters of force. "In other words, the principle of adaptation, as one of the main parts of the theory of evolution, should be separated from the study of the geographical distribution. . . ."⁸ As to this assertion, I agree that plants migrate and select in the same sense that men migrate and select, but I can not agree that a species never adapts itself in such form as to transmit the results of adaptability.

"Environment has only selected the suitable forms from among the throng and has no relation whatever to their origin."⁹ "Natural selection . . . causes survival of the fittest; but it is not the survival of the fittest of individuals, but that of the fittest species, by which it guides the development of the animal and vegetable kingdoms."¹⁰

These last two quotations contain sweeping assumptions and all of them taken together must be interpreted as saying that evolution in plant and animal life arises wholly from accidental changes, that is, from mutations which are matters of accident, having no preceding cause, therefore beyond us as breeders to investigate or change. This would naturally deprive agriculturalists

⁶ DeVries. Plant Breeding. Page 24.

⁷ *Ibid.*, page 322.

⁸ *Ibid.*, page 345.

⁹ *Ibid.*, page 352.

¹⁰ *Ibid.*, page 9.

and stockmen of the right to hope for the adjustment of a species to new surroundings in which at first it does not fit.

It is hard to comprehend a philosophy which recognizes the old conception of species yet breaks it into countless "elementary species," and at the same time claims absolute constancy for the characters of the elementary forms. If mutations, which all agree may occur, do not come from irritations of environment, internal or external, from what cause may they arise? Must we say that each plant with an observable unit character is a species and, with Linnaeus, that it always was? My observations do not allow of this thought; neither have I ever seen an accident in nature. The only observed accidents of nature, when known, have always been found to have direct natural causes. I could not work with faith upon plant breeding if I could convince myself that any plant was ever the result of an accident.

The fact that plants mutate and that new types arise in regular mathematical relations due to cross breeding, and that selections from individuals give the correct methods of breeding, seems evident, but I have seen nothing to convince me of absolute constancy of either individual or species. Mutation seems to be just a good name for a grade of natural changes and no more. We have still to look for the causative feature in the environment behind the mutation or change of character; and, if we wish to improve upon agricultural species or varieties or individual strains, we must select from among the occurring changes, whether we call them mutants, elementary species or unit characters. The demonstrations of mutations and the new knowledge that arises from a better understanding of the laws governing union of and correlation of characters must eventually greatly facilitate the studies looking toward an understanding of direct special causes for the changes which occur in the evolution of a type. DeVries' original experiments were of immense value, but I believe his philosophy of constancy to be

exceedingly bad. There seems to be no observation or experiments which can be interpreted as substantiating that phase of his writings. While DeVries' examples all point to the minutest types of variation and change, his philosophy of constancy is directly opposed to this thought and would dash all of the hopes of the average man doing any work looking toward the improvement of strains, or types. Yet, previous to the last few years, since college men have been given sufficient funds for experiment, it was the practical farmer and stockman who had done the work in improvement of agricultural sorts and races in stock, and most of the improvements have been made from pedigreed strains of rather pure type whether we speak of vegetables, fruits, cereals or cattle. I know this statement will meet with objections, but I believe it will be found to stand upon good bases when we remember the work of our horticulturists and best breeders of animals. For example, Wellman, Haynes and Houston used plant gardens for the production of individuals from individual mother plants in wheat before De Vries, Nilson or Hayes worked. I can not convince myself, after studying the results and observing the work of Mr. Haynes in later years, that he or the other two men got results only from their first selections. Each of these men have time over and again told me that they selected only the best strains from the progeny of the best individual and that each year their crop had improved in the direction along which they worked and that it maintained itself reasonably well in the field. This, of course, DeVries would answer was only the result of "fluctuating variations." My studies do not convince me that he is correct. Undoubtedly, there are countless variations which may occur in the field crop which it is folly to assume may be detected and classified as permanent or fluctuating. Certain it is, that Darwin argued for the great stability of certain inbred stocks and that forces of heredity are stronger than those of variation. It is equally true that the longest pursued

experiments of American agriculture show that pure strains or varieties of close-bred stocks, especially those of cereals, are sufficiently stable to be held to form under intelligent culture, but none of us, as yet, can say further than this. If we maintain and can agree that a mutation or change has a natural cause and that no plants are stable, constant in character, it is nevertheless not necessary to be assumed that the burden of proof rests entirely upon Darwinians.

What evidence may be placed over against the theory of constant elementary species? Well, as an hypothesis, when carried to the limit, an elementary species becomes a unit character and when unit characters are well known they will as in the case of the units of matter, perhaps become so intelligible as to be recognized as the least conceivable element of natural force which makes for union and which in case of the organic units may make for a heritable change. This, the writer believes, is the ultimate result to which most of DeVries' facts and observations point, and my own observations, together with those of others, as I understand them, teach that whenever physical and chemical conditions are changed in the least, within or without the plant, some variation will occur in plant substance, and that this may result in a change in the progeny which may be fixed or modified sufficiently to serve agricultural purposes, if the conditions which originate it may be ascertained and reasonably maintained. It is but reasonable that some changes should approach greater stability than others.

The practical breeders of America, years before the work at Amsterdam and Svalof commenced, developed hundreds of varieties; and though I have worked with many varieties and strains of wheat, flax and potatoes and have found them all reasonably stable if given generally stable environment, I have never yet been sure that I have seen a stable plant or stable strain or variety. Some change is continually taking place and any change, I think, may be fixed just in proportion as we know the conditions which originate it.

This brings me to the statement of a principle of agricultural cropping which, though generally recognized by breeders for years, yet needs to be much emphasized by those who would improve a character of a plant or a plant strain, or maintain a standard of a general crop; namely, the condition under which a character originated or is being originated must be maintained or approached. It is all the more important to hold this feature of cropping well in mind, now that the conception of unit characters and of mutations and of new methods of experimental breeding has proved to be of such fertile aid in the production of new types. Much of the value of agricultural, commercial cropping rests upon the permanence and general use of a few well-maintained or established varieties as against the miscellaneous use of many varieties; thus, for instance, a district or country gains its reputation for a particular crop not by the use of many varieties, as, for example, of wheat, or oranges, but by the use of a few which by careful work are held to their characteristic qualities. It is quite possible to swamp agriculture by the creation of too many forms. Stable industries are built up about reasonably stable crops.

After a new type or a new character in a type has been obtained, new conditions, if they include the essentials of the old conditions, may readily bring about the addition of new features whether we have in mind calling these new features fluctuating or elementary. If the originating conditions are lost or are not maintained, the type should degenerate or retrograde, lose character, and I am quite convinced that it always does. Herein lies the working basis for every-day cropping and breeding. This line of argument is equivalent to saying that so-called fluctuating variations may be maintained and built upon as foundations for important hereditary changes, whether, in writing, we term the resultants mutants, sports or simply elementary units or species. It is equivalent to arguing that the principle of adaptation may

be active in nature even though mutations are observed. Or, perhaps plainer, it is equivalent to saying that mutations themselves may be nothing more or less than adaptations, adaptive changes. I think breeders who ignore this thought in their work will eventually find themselves without a basis for well-founded experiments.

I have been brought to these conclusions chiefly through careful observations upon my own selection and breeding plots, upon which for a number of years, I have attempted to hasten the survival of the fittest, or of the unlike, through the development of or heightening of the action of plant disease for the purpose of eliminating the weak or unfit.¹¹ The method of field work consists essentially in constant culture or cropping to the crop under consideration and in promoting, in every way possible, the development and action of the disease under consideration; and at the same time using all available methods of breeding and selection under these heightened conditions of disease. The method has given marked results when applied to wheat *vs.* wheat rust; wheat *vs.* wheat smut; flax *vs.* flax rusts; flax *vs.* flax wilt; and potatoes *vs.* seab and blight.

My observations along these lines have been such that I have no fear but that the future will find me right in the assertion (1) that mutants may be so insignificant and numerous as to be unrecognizable and thus fall directly into the class called by DeVries, "fluctuating variations"; or (2) that they may be induced in a mixture of a great number of varieties of a species at one and the same time because of the same environmental causes; or (3) that, in some cases, "fluctuating variations" are of such nature and worth as to allow results to be obtained

¹¹ The full details of the methods and details of the results can not be furnished in the limits of this paper. The method of work has been outlined in the annual reports of the North Dakota Agricultural Experiment Station and in an address delivered before the meeting of farmers and stockmen at St. Louis, October, 1903, and further reported upon in vol. I, page 131 of the report of this association, and also in the Proceedings of the Lansing Meeting of the Society for Promotion of Agricultural Science, p. 107, 1907.

in mass breeding of as great importance as any that may be hoped to be obtained by looking for a single mutating type evolved through the method of DeVries.

I am unable to affirm whether disease resistance, immunity to disease, is structural or physiological. I believe the latter, for I have been able to develop it in varying degrees of perfection in every strain of potatoes, wheat or flax with which I have worked, and for all of the diseases noted, while the method used has always remained the same. There is also good structural evidence that it is physiological, as shown in the structural changes caused by the entrance of disease-producing organisms within the tissues of resistant hosts. I have worked with pure strains, centgener progeny, from individuals, with the progeny of cross bred plants, with mixed strains and variations in bulk, and with bulk selections of centgener origin, side by side, and resistance has come at approximately the same rate and grade for a variety through any one of these methods. When the conditions of disease production and conditions of infection for each plant have been held so as to be constant factors, the bulk method of selection, as, for example, selection to seed weight, color and form from a disease plot of flax, has given final hereditary resistance as rapidly as individual selections from the progeny of individuals of the same strains. The only cases in which resistance has developed irregularly in such strains of flax or wheat have been found to be due to impurity of type or to irregularities as to the constancy and amount of disease infection, or to irregularities in the conditions promoting the development of disease.

Resistance to the diseases named for the crops named can be developed in any variety or strain by either method of selection noted. In any case, it may be increased by every proper selection year by year, just in proportion to the perfection with which the disease conditions of elimination are maintained.

The method works in potato selection, where the process

of propagation is by cuttings or buds and hence only a condensed individual life. It works in wheat, which seems quite guarded in its closed or individual fertilization. And it works in flax which, though usually self-fertilized, is, no doubt, much given to intercrossing in the open.

If the proofs rested only upon flax, where there are many possibilities that open crossing might give rise to the various Mendelian types, homozygotes, heterozygotes, etc., the evidence might be thought to be thrown into question, but even there, when crossing is promoted, while there is every evidence of the occurrence of resistant or non-resistant types among the crosses, resistance is seldom found to approach the immune type upon the first possible selection. In other words, if mutants do occur in flax through natural crossing or self-fertilization, so far as our experiments are concerned, these resistant forms are found to act exactly as do those which may be developed from ordinary types of non-resistant flax.

With this crop, one finds little, if any, resistance to wilt and to rust in the best seed strains of southern Russia or in the best seed strains from the new lands of our northwest or in the best fiber strains of the disease-free districts of northwestern Russia, though it often crops out in the general seed samples from the worst disease infection regions of central Russia. If mutants were without cause and constant, one ought to find them as readily in the seed of one district as in another. One can, however, build resistance upon the least resistant of these strains, if the work is started upon a graded scale of disease infection which is increased year by year. If the seed is placed under too heavy conditions of wilt production, the plants are all killed in embryo or young stages of growth and nothing is gained. If, however, scrubs or runts may be saved from the first season under weak disease infection and a graded infection is followed thereafter, in approximately five years one may bring these strains of seed to such a stage of resistance that ordinary agricultural methods of cropping will maintain

them upon the most flax-sick areas. But whether these resistant strains have been selected through crossing or by this gradation method from bulk seed or from individuals taken from a known pure strain, I have found that abrupt heightening of disease conditions of too great violence may undo the whole work.

It holds as well for wheat when speaking of rust attacks. Under uniform conditions of rust infection, all wheats arise rapidly to a stage of marked resistance to general uredospore infection whether caused by the type *Puccinia graminus* or *P. rugigo-vera*, which resistance seems to be characteristic for each variety concerned, but may then fall a ready prey to sudden attacks introduced by properly conducted aecidial infection. All this points to the rôle played by the irritations of environment, which either govern the appearance of mutations or produce other changes which are very worthy of the breeders' and croppers' attention; and allows one to ascribe much more merit to methods of mass selection and breeding in cereals than the DeVriesian doctrine of constancy of elementary species will allow one to assume.

No phase in this argument touches upon the ultimate causes of disease resistance or immunity. But the facts do point quite clearly to the probable influence of chemical agencies, perhaps toxines, arising from the direct existence of fungus attacks upon the hosts. In my mind, there is not the slightest doubt but such attacks originate heritable resistance, in much the same sense as MacDougal's chemical injections upon ovaries are supposed to have originated new types. If later experiments prove MacDougal's observations to be well founded, the results will be of far reaching importance. If these suppositions that fungus attacks upon the host may induce fungus-resistant qualities in the progeny from the matured ovaries, are correct, no doubt the unit characters, so called (whether simple and definite in number or whether they may be considered as composed of countless and variable elements of the cromatin structures) may be effected, or

may be originated, and one ought, under crossing, to observe the effects in terms of dominant and recessive or cloaked features. Such effects I have observed to occur in flax as against flax wilt and in wheat as against *P. graminus*. Mr. R. H. Biffin, of England, is reported as indicating that this is true in the case of wheat when crossed with einkorn as against yellow rust.

Bateson, in an admirable article upon "Facts Limiting the Theory of Heredity,"¹² would also seem to enroll himself as against any adaptative response to changed conditions as able to account for the origin of such facts as I have observed in my cultures of flax and wheat. Thus we read:

"Though the response to change of conditions may have been direct, it must not be hastily concluded that the response is adaptive. The appeal to direct responses so common in evolutionary discussions of thirty years ago, was made to account for the complex adaptations of organism to environment. It is the total want of any evidence supporting that appeal which has driven most of us to disbelief in the reality of any such claims, and there is nothing in the new evidence, I think, which should shake the attitude of resolute agnosticism which we have thus been led to adopt."

However, to explain such observations as those noted by me in the flax and wheat cultures seems to demand the assumption that additional elements of heritable character arise on account of causes demanding adaptive response. No theory of quantitative subtractions of unit characters already formed would seem to be adequate to account for the observed acquired qualities of resistance. It is probable that only the cytologists are in position to produce direct proof or disproof of the apparently necessary suppositions as to character modifications. Certainly those among them who are experimentally inclined may cut along the line indicated with much hope of uncovering many high lights to breeders. Even here, it may be expecting quite too much that possibly pure physiological qualities should be represented by structural units.

¹² *Science*, November 15, page 649.

WHAT IS A SPECIES?¹

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WE have had in the past not a few interesting discussions upon or controversies over the methods of evolution and the origin of species in this club. We have more or less plausible theories as to the evolution of species, by natural selection, mutation, inheritance of acquired characters, etc., but we have very nebulous ideas as to what species really are. Nothing is more common than the term species; nothing is more uncertain than what species are.

Long after the present topic had been suggested for discussion by the club, I was delighted to learn of a like discussion to be held at the late meeting of the Botanical Society in Chicago. I anticipated that symposium with the liveliest feelings of satisfaction, confidently expecting a brilliant illumination of the whole umbrageous subject, an effulgence of light that would throw into deepest obscurity whatever feeble beams I might myself hope later to cast upon it. But lamentable was my disappointment. I heard some ancient platitudes that the zoologists ceased in despair to consider a dozen years agone, and many anathemas showered upon the botanical taxonomist. They berated him for making such a mess of classification, and hinted very freely that he didn't know much anyhow, and probably never would. If all that was said be true, then indeed the botanical taxonomists are a sorry lot. One distinguished speaker declared they had made no progress since the time of Linné, and he rather seemed to desire that the whole tribe might be banished to some desert isle where there is neither vegetable life nor printer's ink, that they might no longer trouble the ecolo-

¹ A paper read before the Biological Club of the University of Chicago.

gists and physiologists, *et id genus omne*. But I can not escape a harrowing doubt as to what the learned speakers' vocations in botany would be, if the maligned taxonomists had not got in their diabolical work. Possibly they would distribute an herbarium with each lucubration, that their readers might know what they were writing about; or possibly they might undertake to name their own species, for I have seldom known the morphologists to escape the mihi itch on very slight infection.

¶ I sympathize with the physiologist or ecologist, who after he has written a luminous paper on a Crataegus or Viola, or Rosa, or Opuntia, endeavors to ascertain the proper name for his plant; but I do not sympathize with his objurgations against the whole tribe of species makers. There is a deal of pseudo science, unripe science—were it not undignified I would characterize some of it by an expressive monosyllabic word suggesting decomposition—published about species by the taxonomists, but I suspect that there is also a large deal of like obnoxious material lying at the doors of the physiologists and ecologists and morphologists. But that fact does not make taxonomy or ecology anything less of a science, nor the work of able men in either less valuable. I am a little weary of hearing from narrow specialists in other departments of biology constant condemnation of the taxonomist, and I have been hearing such for the past fifteen years from men who should know better.

Now, as one who has been guilty in the past twenty-five years, let me say it humbly, of naming and attempting to describe ten or twelve hundred so-called species and genera, I beg leave to make a few remarks about species. You may properly accuse me of being one of those degenerates the taxonomists and, as his tribe is not well represented here to-night, I may be permitted to present his side of the case. I hold no brief for the criminals, but, as one of the accused, I would present my own defense and my own views.

The question, What is a species? has been asked re-

peatedly, I may say continuously, since the time of Linné, even if our friends the botanists have been somewhat somnolent of late. We have had no satisfactory answer, for the simple and very good reason that there is none, and never will be. If we could go back to the happy Cuvierian or even Agassizian days and throw the whole responsibility of their definition upon the Creator, seeking some revelation in Holy Writ, it would save us much useless worry. But, as we have long since learned that species, like Topsy, just grew, we have and always shall have as great difficulty in deciding when varieties and races become species as we have in determining when a puppy becomes a dog or a lamb a sheep.

Let me premise further with the statement that true taxonomy is the most advanced and difficult of all biological science. O modest claim is it not? But I think that you will readily admit that evolution, as a science, is the highest end of biology—and taxonomy is merely the graphical expression of evolution. If, then, we do sometimes baptize a score of hawthorns, or bedbugs, or coyotes, where the breeder or ecologist thinks that he finds but one later, we crave your sympathy and your aid, not your scorn and contumely. The breeder may get all variations between the domestic ox and the American bison, and they will breed reasonably true by artificial selection, but the assertion that *Bos americanus* and *Bos taurus* are one and the same species would be preposterous. Is it not possible that some of our learned breeders of plants and animals may be in error themselves in their views of "species"?

All classificatory terms are impossible of exact definition. Their use always has and always will depend upon the consensus of opinion of those best qualified by wisdom, experience and natural good sense. They will never become stable; we shall never cease to amend, to change, to repudiate old and propose new, because we shall never reach the final summation of science. We can only hope that all changes shall be for the better, shall be nearer the

real truth; that incompetent and inexperienced taxonomists shall be ruled out of court, even as incompetent anatomists and cytologists are disbarred. The unfortunate thing is that we taxonomists have so bound ourselves in a snarl of laws and by-laws that we are compelled to incubate and wet-nurse every premature and monstrous taxonomical imbecility till it dies a natural death, whereas those of other biologists are promptly strangled or thrown out into the cold to die of inanition. Let us hope that we may escape from some of that snarl, or at least that we may cut some of the bonds which hold us too tightly.

To discuss our subject in all its details and bearings and from all view-points would require, not one evening, but many. Permit me, therefore, to offer for your consideration certain axioms of evolution—theories or hypotheses if you prefer to call them such—bearing more or less closely upon the question, What is a species? Some or all may be familiar to you—I do not know whether all have been in print or not—but, such as they are, they are all based upon my own observation, and, so far as that goes, I am prepared to defend them, and will endeavor to do so later if there are any you repudiate, as perhaps there are.

1. The only biological entity is the individual, and the individual is inconstant.
2. The value of specific characters is dependent upon a number of interrelated and inseparable factors, the chief of which are environment and heredity.
3. Accumulated heredity may outweigh natural selection or environment, and *vice versa*.
4. A crescent phylum is more variable, more plastic than a long established one; that is, time is always an element in the fixation of characters and the limitation of variation, and the length of time is dependent more or less upon the strenuousness of environment and selection, and the plasticity of the type.
5. New phyla arise from crescent phyla, never from decadent or even dominant ones.

6. The decadent phylum may present as unstable saltations, generic or even higher characters of allied dominant groups; that is a character of generic or even family value in a dominant group may be merely an individual variation in a decadent one.

7. The members of a dominant group are, *ceteris paribus*, more closely adapted to their environment, their characters less variable, their geographical distribution more restricted. That is, species of dominant groups may be safely based upon less distinctive characters than those of a crescent phylum.

8. It follows that senility and decadence are the attributes of species, families and orders, as well as of the individual.

• 9. The older the genus or allied group of species, the more restricted, apparently, is fertile hybridity. For example: The genus Rhinoceros is an old one that has been but little modified since early Miocene times; I have never learned of cases of hybridity between living species. Equus arose in early Pliocene times with all its essential modern characters; hybridization between all its living species is not difficult, but the hybrids are infertile. The genus Bos, while beginning in the Pliocene, did not attain full development till Pleistocene times; its numerous species are continuously fertile in all combinations. Rhinoceros is long past the zenith of its evolution; its highest specialization was in the Pliocene, or at most Pleistocene. Equus, too, is past the highest point of its development, perhaps, but not far. Bos, on the other hand, is a dominant or crescent type; its maximum specialization is in the present time.

10. Secondary sexual characters are transmitted to the opposite sex, unless of positive disadvantage. Varietal and specific characters, in the natural course of events, are more or less unisexual at their inception, and the constant tendency is for the characters of one parent to be transmitted to offspring of both sexes, even when such characters are apparently useless, as seen in the rudi-

mentary mammae of the human male, which, indeed, sometimes become of functional use.

11. Secondary sexual characters are more numerous and less stable in the male than in the female; that is, female sexual characters, whether primary or secondary, may be of generic or even family value in groups wherein like characters in the male are merely specific or even individual. I am aware that some modern naturalists would discredit sexual selection, but, until some hypothesis is given to replace it, I must still continue to believe that sexual selection is necessary to account for secondary sexual characters.

12. An organ once functionally lost is never permanently regained by natural selection or any of its hypothetical substitutes. A hexadactyl species of *Homo* or *Felis* is impossible.

13. Giantism in any group is an indication of approaching decadence; giants never give origin to dominant phyla of smaller average size.

To these I may add, as an article of faith, one more:

14. Fertility depends chiefly upon the inheritance of physiological characters. A modification in the behavior of the sperm and germ cells may affect fertility even before structural characters have become much affected, and *vice versa*. Human males and females, as we all know, are sometimes infertile with each other, though each may be entirely fertile with some other; an extension of this infertility to races would produce what the taxonomist would accept as species. I furthermore believe that the accumulated inheritance of physiological characters may and does produce determinate lines of evolution, that is, orthogenesis, which may go on into hypergenesis, if I may use this term to indicate that hereditary momentum which results in over-development of organs. I account for this accumulated heredity by the action of past environment upon the organism, that is, Lamarckism. I am also quite aware that I am with the minority in the acceptance of Lamarckism as the chief causative prin-

ciple of the origin of species. I am told that the direct effect of environment in modifying germ characters can not be proven, and I retort, neither can it be disproven. Paleontologists almost universally, and taxonomists generally, are Lamarckians, that is, those who deal chiefly with range and distribution, time and space; laboratory biologists, on the other hand, are almost invariably opposed to the theory of the transmissibility of acquired characters. We can see no alternative hypothesis that will meet the requirements of the classificationist or the paleontologist, and we respectfully submit that the experiments of a few years or even scores of years are trivial in comparison with the natural experiments that go on through tens of thousands of years in the origin and fixation of species. From my little water-garden some years ago I took some plants of the common water hyacinth and planted them in the ground. I was surprised to find that they grew luxuriantly, but that they did not develop the peculiar bladder-like swelling of the leaf stems; when I again transferred some small offshoots to the water, they promptly redeveloped them. The plants immediately changed their structure in adaptation to their terrestrial or aquatic environments, and doubtless they would do so after many years of isolation. But had the water hyacinth been cultivated as a garden plant in the soil since the time of Pliny I believe that the terrestrial character would have become fixed, and, after all, two or three thousand years is a very short time in the history of plant species. The Weismannians have been compelled to recede a long way from their first position of the absolute and eternal distinction between germ-plasm and body-plasm; perhaps we shall yet find an intermediate place that will satisfy us all.

In claiming a high degree of importance for physiological characters and physiological isolation in the formation and preservation of species I need not say that the term physiological is merely a confession of ignorance. All physiological function must inevitably dependulti-

mately upon structure. Two cells absolutely alike must doubtless function quite alike. But I doubt if there ever are any two cells *quite* alike. We are already learning, if I am correct in my understanding of the claims of McClung and others, that even minor, so-called specific differences are discoverable in the cell, in some groups at least. There is, of course, no such thing as a purely physiological species, for changed structure must underlie all variation, though we may not be able to discover the differences. I can conceive that additional or modified chromosomes in the germ cells of the greyhound might indicate a partial physiological isolation which has preserved this race of dogs almost undefiled through more than three thousand years; certainly man has not been the cause of its preservation.

Now, if the foregoing theses or hypotheses be true, or even if the greater part of them be fundamental principles of variation, it follows that the definition of species must be made for each and every one that exists or has existed; that a specific character in one group may be merely varietal in an allied group, on one hand, or generic in another, on the other hand. Or, aphoristically, every species, as we know to be the case with every genus, is a law unto itself. And this is, practically, the working rule of every competent taxonomist, though of course many sad errors are made in its application. And it follows that he who is best qualified to propose and name species, or to criticize those which have been proposed by others, is one whose acquaintance is widest with living forms and with the laws which underlie their evolution. He must not confound genetic with adaptive characters, for phylogeny is the sole end of taxonomy.

Since we can not give an answer to the question, What is a species? let us analyze briefly some definitions of the past:

1. A species is a form of life which breeds true to itself. The Jewish race has bred true to itself, as indicated by its distinctive physiognomy, since the time of Rameses; and

the Bourbon nose is characteristic of that family. Ergo, the Jews and the Bonapartes are distinct species of *Homo*!

2. True species are incapable of fertile hybridity. The example of the Catalo, the fertile hybrid between *Bos americanus* and *Bos taurus*, will suffice. The domestic dog interbreeds freely on the plains with the coyote, and no one doubts the specific validity of *Canis latrans*, whatever we may think about coyotes in general. The domestic cat, according to Pocock, is a hybrid between the wild cat of England and the wild cat of Egypt, with a distinct tendency to vary along ancestral lines after centuries of fertile hybridity. The domestic races of dog freely interbreed, and yet we are quite sure they are the derivatives of several wild species of *Canis*. May not fertility, as a physiological inheritance, account for the preservation of their distinctive types, notwithstanding man's artificial selection? Is it probable, for instance, that the Boston terrier hybrid will continue longer than the fad of its breeding remains undiminished?

3. A species is a type which varies only within narrow limits. The jungle fowl is fairly constant in nature. Its extraordinary variability is seen in the domestic fowls, whether they be derived from a single or several wild species. And the doves are still better examples. A turkey and guinea-fowl, on the other hand, though they have been domesticated for centuries, vary but little from their ancestral types.

There are other definitions. But, you say, if I accept no definition of species what rules do the taxonomists have who "make" so many thousands of them? We must have some, and we do have them, even if we are so often accused of depending on whim and imagination. And these are mine: Forms of animals which present distinct assemblages of characters, in form, color and arrangements of parts under natural conditions, which are recognizable from descriptions and figures, should receive distinctive names and be catalogued, provided, of course that the assemblage of characters includes all onto-

genetic changes. If, in the examination of abundant material from different natural environments, we find these characters fairly constant, the forms may properly be called species; if not, varieties or races. No perfect specific description can be drawn from a single specimen or from a few even, and the skill of the taxonomist is conspicuously shown in his ability to distinguish between variable and fixed characters, between the essential and non-essential, in other words, between old and new characters. Some taxonomists—and I know such—remain, after many years' experience, unable to dissociate individual from specific or generic characters; they describe species as they would describe the physical features of a tree or of a rock—and they are the ones who deserve the condemnation of other workers. Are such workers confined exclusively to our branch of biology?

In nature the interrelated factors, of which environment and heredity are the chief, are normally in a state of substable equilibrium—variations within given groups are within certain fairly definite limits, because the factors of variation are. If, however, the cumulation of any one factor, either naturally or artificially, occurs, the variations become inconstant and the limits of variation are changed. The wild pigeon in nature, for instance, is governed by fairly constant conditions, and its variations are small. Its domestic varieties, were they existent in nature, would not interbreed, and would be good species. Breeders, I think, lose sight of such things when they say, as some do, that they can produce specific characters, that is, characters which are deemed of specific value by taxonomists. They can do nothing of the sort. You may break down by changed environments and artificial selection what would be real specific characters under natural selection and natural environment, but you do not make species thereby. Time and fixation by heredity, I believe, must always be taken into account in determining varietal, specific or generic characters. Mutations of *Oenothera lamarckiana* were found growing wild by de Vries, es-

caped from some garden. Under cultivation he continued to breed them, and produced others. But his plants were all under abnormal environments. Dr. Lutz, whom we all know, is doing some exceedingly interesting experiments upon certain small flies, *Drosophila ampelophila*. He has produced some remarkable sports or "mutations," and the surprising thing is that he finds that such sports breed true, that there is an apparent loss of fertility between them and the normal forms. But I think it is absolutely certain—and I speak as an entomologist fairly familiar with flies—that it would be impossible to produce species of his sports, even though they were bred for a thousand years. As some of us know, polydactyl cats are rather abundant in some parts of Connecticut, breeding true—but who believes in a species of six-toed cats? It is rather unfortunate that breeders confine their attention almost exclusively to plastic forms, that is, to geologically recent types. Let some one try experiments with archaic forms and watch the results.

Experimental breeding and ecology are the two fields of biological research which promise most at present; they will doubtless contribute not a little to our knowledge of the methods of evolution, and correct not a few errors in taxonomy; but I say, with full deliberation, that experimental breeding without a wide knowledge of taxonomy will lead to false conclusions and be comparatively barren of results. The experimentalist, of all men, must be well acquainted with varietal specific and generic characters in the groups which he studies, or he will be working blindly. And I am sorry also to say that some of the severest criticisms of taxonomists and taxonomy have come from some of these men.

SHORTER ARTICLES AND CORRESPONDENCE

THE INHERITANCE OF THE MANNER OF CLASPING THE HANDS

If the hands be clasped naturally, most people will put the same thumb—either that of the right or of the left hand—uppermost every time. The position assumed apparently has no relation to right- or left-handedness, although, as will be shown, a small majority put the right thumb uppermost. Some time ago letters were sent out asking for data concerning the manner in which the different members of families clasped their hands. Among the many generous replies was one from Professor J. Arthur Thomson, of Aberdeen, Scotland, giving data for about 600 individuals. It was intended that the hands should be clasped with the fingers of each hand alternating; but this was not made as clear as it should have been, and some of the correspondents clasped their hand with all of the fingers of one hand between the thumb and index finger of the other. This confusion does not exist in Professor Thomson's data. Accordingly only they are discussed in this note. The accompanying table gives a condensed analysis of the data, R and L standing for right thumb uppermost and left thumb uppermost, respectively.

Parents.	No. of Families.	Offspring.					
		Male.		Female.		Total.	
		R.	L.	R.	L.	R.	L.
♂ R. × ♀ R.	75	71	23	95	40	166 (72.5 %)	63
♂ R. × ♀ L.	49	33	22	28	27	61 (55.5 %)	49
♂ L. × ♀ R.	53	46	24	40	40	86 (57.3 %)	64
♂ L. × ♀ L.	36	24	29	22	34	46 (42.2 %)	63

It is evident that the mode of clasping the hands is inherited. It can scarcely be acquired by imitation as it is too slight a thing to be noted unless attention is called to it. The thumb position is usually quite constant in very young children. However, it does not seem to follow the Mendelian law, as neither position breeds true. The data show no significant sexual dimorphism,

61 per cent. of the males having the right uppermost and 58 per cent. of the females; 59 per cent. of the parents and 60 per cent. of the offspring put the right uppermost, so that there does not appear to be any reproductive selection. The coefficient of association between parents of 0.02 demonstrates the lack of assortative mating. This last conclusion is in sharp contrast with the results concerning other characters in man.

There are a number of somewhat similar problems in the lower animals which are of importance in the study of evolution. Thus, the males of the common black cricket (*Gryllus*) usually keep the right tegmen over the left. This results in one set of sound-producing organs being functionless. In the closely related *Locustidae* there is only one set of sound producing organs and the tegminal position is fixed. It would be interesting to know if mutations to the other position occur. The fish *Anableps anableps* has the anal fin modified into an intromittent organ adapted for sidewise motion. On about three fifths of the males it can move to the right and on about two fifths to the left. (AMER. NAT., xxix, pp. 1012-1014.) A similar state of affairs exists in the females, but with the relative frequencies reversed. Copulation is effected by a right male at the left side of a left female and *vice versa*. Whether the species will eventually split up into two on the basis of this character or not would seem to depend on how the anal-fin-twist is inherited. However, if the tendency to twist to the right or to the left be inherited as a character apart from sex there would seem to be no chance of two varieties or species being formed, as each mating is between opposites. The reversed position of the nerves in the optic chiasma of fishes was found by Larrabee (*Proc. Am. Acad. Arts and Sciences*, xlvi, No. 12) not to be inherited.

FRANK E. LUTZ.

NOTES AND LITERATURE

ICHTHYOLOGY

Ichthyological Notes.—One of the most valuable papers on the habits of fishes is a study in sexual selection by Cora D. Reeves, "On the Breeding Habits of the Rainbow Darter (*Etheostoma caeruleum*)."¹

Miss Reeves, a graduate student of Professor Reighard in the University of Michigan, has carefully watched the breeding habits of this dwarf perch, a species in which the males are most brilliantly marked with blue and scarlet.

It appears that the females do not select the brilliant males, that the oldest and strongest males are most brilliant, that the males know the females only by their color, that they mistake young uncolored males for females, and that the bright-colored males frighten away the younger ones by the display of their gaudy fins and by blows of the tail or head. The brilliant males were successful in pairing in 60 per cent. of the observed cases. The sexes as usual in vertebrates are equal in number, and the blue and scarlet colors belong to the males alone, these being most brilliant at the beginning of the season—about April first.

These observations give no support to the theory that the females choose the gaudy males. It appears however that the most brilliant males are the oldest and strongest, and that they leave most descendants. This form of the theory of sexual selection would seem to be applicable to bright-colored fishes generally.

MR. E. W. GUDGER² shows that the hammer-head shark (*Sphyma zygantha*) feeds on sting-rays and that the mouth and body of the shark are often full of broken-off stings, 50 of these being extracted from a single shark 12½ feet in length from Beaufort harbor.

MR. HERBERT W. RAND³ discusses the functions of the spiracle of the skate, one of these besides the usual function of respiration is to keep the eyes clean by a jet of water.

¹ *Biological Bulletin*, XIV, 1907.

² *Science*, XXV, p. 1005.

³ *AMER. NAT.*, XLI, p. 288.

MR. CHARLES R. STOCKARD⁴ describes the development of the eggs of the killifish (*Fundulus heteroclitus*) in a solution of magnesium chloride.

In this fluid the young fishes develop as one-eyed monsters, the two eyes coalescing into a single median eye. This eye has a single lens. A mixture of sea-water with this solution gives the same effect. The influence of the magnesium salt is therefore supposed to cause the strange development of the eyes.

IN the *Proceedings* of the U. S. Nat. Mus., XXXIII, Jordan and Herre describe the cirrhitoid fishes of Japan, describing one new genus, *Isobuna*.

IN the same proceedings, Jordan and Richardson describe a new killifish, *Lucania browni*, from a hot spring in Lower California.

IN the same proceedings, Eigenmann & Cole record the species of South American Characin fishes in the National Museum and in the Museum of Indiana University, with numerous plates and descriptions of new species.

IN the same proceedings Professor Edwin Linton describes the worms parasitic on fishes of Bermuda.

IN Bureau of Fisheries Document No. 627, Mr. F. M. Chamberlain records his observations on the salmon and trout of Alaska.

Among other matters of interest it is noted that a majority of the young red salmon spend their first winter in the lakes, while the other species leave early for the sea. It is long known that the red salmon never enter a stream that has not a lake in its course and that they always spawn in streams above a lake. The other four species of Alaska salmon have no special relation to lakes.

Mr. Chamberlain's observations do not confirm the "homing theory." Marked salmon fry from the Naha River appeared as adults at Yes Bay. Most of the fishes however seem to return to the parent stream, from which they probably never wander very far.

Four years is shown to be the usual age of maturity of king salmon and red salmon, while the humpback is probably adult at an earlier period.

⁴ *Archiv Entwicklungs Mech.*, XXIII, p. 249.

Mr. Chamberlain's paper contains many valuable facts in relation to the life of salmon, the fruit of nearly four years observation in southern Alaska.

In the *Proceedings* of the Biological Society of Washington (XX, 1907) Evermann and Goldsborough give a check list of the fresh-water fishes of Canada. 145 species are recorded, with a complete list of localities.

In the *Proceedings* of the same society (XXI, 1908), Dr. Evermann describes two new fishes (*Fundulus meeki* and *Salmo nelsoni*) from streams in lower California. The last-named, an isolated troutlet from Mount San Pedro Martir, is especially interesting as occurring farther to the southward than any other known trout. It is apparently an offshoot of the rainbow trout of California, *Salmo irideus*.

In the same *Proceedings* (XXI, 1908) Jordan and Grinnell describe another isolated dwarf waif among the trouts, as *Salmo evermanni*. This troutlet occurs on the headwaters of the Santa Ana River, on Mt. San Gorgonio, in San Bernardino Co., California. It is shut off from the parent form, *Salmo irideus*, by a waterfall, and in isolation it has undergone considerable change. Its origin is parallel with that of the three species of golden trout of the high Sierras. *Salmo evermanni*, living on gray granite, is dull in color with none of the scarlet shades of the golden trout.

In the same connection Jordan and Snyder describe and figure the great Kamloops trout from Lake Kootenay in British Columbia. The specimen figured, sent by John P. Babcock, Fish Commissioner of British Columbia, weighed 22 pounds. This species is an offshoot of the steelhead trout, *Salmo rivularis*.

In the *Bulletin* of the Bureau of Fisheries, XXV, for 1906 (October 25, 1907), Jordan and Snyder record a number of new species from Hawaii, with several plates, four of them in color. The new species are *Caraux dasson*, *Ariommus evermanni*, *Rooseveltia aloha*, *Thalassoma neanis* and *Scaridea aërosa*.

The advent of Japanese fishermen in Hawaii has caused many very rare deep-water fishes to be common in the markets; among these is the type species, *Rooseveltia brighami*, of the genus named for the naturalist Theodore Roosevelt. This species, scarlet and gold, is one of the most brilliant in Hawaiian waters.

IN the same *Bulletin*, Dr. M. X. Sullivan discusses in detail the digestive tract in sharks.

IN the same *Bulletin* (Vol. XXVI, 1907), Evermann and Goldsborough give a catalogue of the Fishes of Alaska, with many plates, based primarily on the collections made by the Albatross in 1903, under the direction of Jordan and Evermann. The following new species are described and figured:

<i>Polistotrema deani</i>	<i>Blennicoltus clarki</i>
<i>Sebastodes swifti</i>	<i>Pholts gilli</i>
<i>Icelinus burchami</i>	<i>Lumpenus longirostris</i>
<i>Coltus chamberlaini</i>	<i>Lycodes jordani</i>

Many corrections in synonymy are made. One of these the present writer does not accept. He believes that the two large rays of Monterey Bay, *Raja stellulata* and *Raja rhina*, are both distinct from *Raja binoculata* and from each other. The authors have overlooked *Bryostemma tarsodes* described from Alaska by Jordan and Snyder.

This paper is a most useful one to students of Alaskan fishes.

IN the *Journal* of the Imperial Fisheries Bureau of Japan, Dr. K. Kishinouye describes the natural history of the sardine of Japan (*Sardinella melanosticta*) and the related species.

For some unexplained reason, he unites the Pacific herring *Clupea pallasi* with the Atlantic *Clupea harengus*. New species of sardine are described under the names of *Clupea immaculata*, *Clupea okinawensis* and *Clupea mizun*. These belong to *Sardinella*, and the last two are from the Riu-Kiu Islands. A new anchovy is described as *Engraulis koreanus*. This should belong to the modern genus *Anchovia*, being very different from *Engraulis japonicus*.

IN the *Journal* of the College of Science, of the Imperial University of Tokyo (XXI, 1907), Professor S. Hatta of Sapporo describes the gastrulation of the embryo of the lamprey.

IN the *Proc. Zool. Soc., London* (1906), Professor Bashford Dean has notes on living specimens of the Australian lung-fish (*Neoceratodus forsteri*). Its movements are notably those of an amphibian rather than a fish.

IN the *Annals* of the Museum of Natal, Mr. C. Tate Regan describes new species from that coast. A species of remarkable interest is a new saw-shark, *Pliotrema warreni*, with six gill-slits.

IN the *Annals of the Museum of Vienna*, XXI, 1906, Dr. Viktor Pietschmann has a valuable record of the fishes collected in a voyage to Iceland, and another to Morocco.

IN *Records of the Australian Museum*, VI, 1907, Allan R. McCulloch describes the fishes and crustaceans taken by the Woy Woy, in deep waters of the Tasman Sea. The most notable of the new species is the sculpin-like fish, *Hoplichthys haswelli*.

IN the *Journal of the Linnaean Society of London*, Mr. A. D. Darbshire describes the water current in the spiracle of sharks.

IN the *Sitzber.* of the same museum, Vienna, Vol. 116, 1907, Dr. Viktor Pietschmann describes new sharks from Japan, *Centrophorus steindachneri* and *Etmopterus frontimaculatus*.

The last species was also taken by Jordan and Snyder in Sagami Bay, in company with *Etmopterus lucifer*. It is one of the smallest of sharks, black, with a milk-white spot on the top of the head.

IN the *Proc. Zool. Soc., London* (1907), Mr. Regan redescribes *Velifer hypselopterus*, a Japanese species not seen in the last fifty years, from three specimens in the British Museum. A second species from northern Australia is described as *Velifer multiradiatus*.

In another paper Mr. Regan brings together the aberrant genera *Lampris*, *Velifer*, *Trachypterus* and *Lophotes*, framing of these a new suborder *Allotriognathi*. These fishes, like the Berycoids have an orbitosphenoid, but no mesocoracoid bone.

Mr. Regan thinks that these fishes are derived from the berycoids, and the latter from forms like the extinct *Ctenothrissa* and *Pseudoberyx*. These observations of Mr. Regan are very interesting and his conclusions seem reasonable. He notes that *Semiphorus* is allied to *Platax* and not to *Lampris*.

IN "Wissenschaftliche Ergebnisse einer Zoologischen Expedition nach dem Baikal-See" (1907), Dr. Leo Berg discusses the sculpin-like fishes of Lake Baikal, the Cottidae, Cottocomorphidae and Comephoridae. The excellent monograph is preceded by a careful account, fortunately in German not Russian, of the osteology of these fishes. Of the 34 species of fishes found in Lake Baikal 17 are endemic, or developed in the lake, not occurring elsewhere. Of these, two genera, *Comephorus* and *Cottocomphorus*, each constitutes a distinct family. Other genera

peculiar to this lake are *Abyssocottus*, *Cottinella*, *Limnocottus*, *Procottus*, *Batrachocottus* and *Asprocottus*. The fauna described in this paper is one of peculiar interest.

IN another paper in the *Annuaire de l'Académie de St. Petersbourg*, XI, 1906—unfortunately entirely in Russian—Mr. Berg discusses the Old World species of minnows of the genus *Phoxinus*.

IN the same *Annuaire*, vol. XII, 1907, Mr. Berg treats in Russian and in English, the fresh-water fishes of Corea. The genus *Longurio* of Jordan and Starks is united by Berg to *Sauvogobio* of Bleeker, and *Fusania* with *Aphyoeypris* of Günther. In another paper Mr. Berg discusses Siberian species of *Rhodeus*.

IN the "Fauna America-Centrali" Mr. C. T. Regan continues his account of the fishes of Mexico and Central America. *Amiurus meeki* is described from Chihuahua, *Moxostoma mascotae* from Jalisco, *Algansea affinis* from Rio Lerma, *Algansea stigmatura* from Rio Grande de Santiago. Several species recognized by other authors are placed in synonymy. In some cases this process exchanges one doubtful opinion for another.

IN *Popular Science Monthly*, LXXI, January, 1908, Dr. Jordan describes "The Grayling at Caribou Crossing," a running descriptive account of the Yukon country, with a dash of angling.

IN *Science*, XXVI, 1907, Dr. Bashford Dean reviews Dr. Eastman's recent papers on the "Kinship of the Arthrodires," Dr. Eastman claims that these mailed fishes are Dipnoans and to this conclusion Dr. Dean enters a "friendly protest."

IN the *American Journal of Anatomy*, VII, 1907, Dr. Dean discusses the structure and origin of the Aeanthodian sharks, one of the most primitive of the extinct forms. Their relationships are with *Cladoselache*, a form having fins of the fin-fold type, and probably the most primitive of known sharks.

IN *Archives de Zoologie Experimental*, VII, 1907, Dr. Louis Fage describes the fishes of the Balearic Islands, with several new species. The paper can be especially praised for its attention to laws of nomenclature, as also for the accuracy and fullness of its accounts of the new forms. Two species are referred to the genus *Eleotris*, a group not hitherto recorded from Europe. Neither of the species, however, belongs to *Eleotris*.

proper. *Eleotris pruvoti* is an ally of *Valenciennea* and *Eleotris balearica* approaches *Gymnoleotris*.

DR. JACQUES PELLEGRIN publishes in the "Mission Scientifique," 1907, an account of the fishes of the high mountain lakes of South America.

IN a weekly Journal, the *Sydney Mail*, Mr. Charles Thackeray gives a readable and accurate account of the game fishes of New South Wales, with wood cuts of the leading species.

ECHINODERMATA

The Stalked Crinoids of the Siboga Expedition.¹—There has recently been published a monograph on the recent stalked crinoids of the East Indies, based on collections made by the *Siboga* which is the first important contribution to our knowledge of the group since the publication of Dr. P. Herbert Carpenter's great work (the *Challenger* report) in 1884.

The first thing to attract the attention of the student of the recent crinoids is the announcement of the discovery of the infrabasals in a species of *Metacrinus*, *M. acutus*, a new species here first described. Dr. Carpenter stoutly maintained that infrabasals did not occur in any species of recent crinoid, and he criticized rather sharply the so-called law of Wachsmuth and Springer, by the application of which the recent genera *Iso-crinus* and *Metacrinus* were shown to be dicyclic. He dissected numerous specimens of various species of both genera, and according to his statements and figures, appeared to conclusively prove their absence. In 1894 the Swiss paleontologist de Loriol discovered and figured infrabasals in a fossil species of *Iso-crinus*, and now Professor Döderlein shows their presence in *Metacrinus*. This discovery by Dr. Döderlein was made simultaneously by the present reviewer in two other species of *Metacrinus* and in *Iso-crinus decorus* and announced in a paper now in press, which had gone through the final proof before Dr. Döderlein's contribution was received. In this the infrabasals of *Metacrinus rotundus* from Japan and of *M. superbus* from the China Sea, and of *Iso-crinus decorus* from Cuba are described and figured.

¹ Die Gestielten Crinoïden der | Siboga-expedition | von | L. Döderlein. | Monographie XLIIa aus | Uitkomsten op Zoologisch, | Botanisch, Oceanographisch en Geologisch gebied | verzameld in Nederlandsch Oost-Indië 1899-1900 | aan boord H. M. Siboga onder commando van | Luitenant ter zee 1^e Kl. G. F. Tydeman | uitgegeven door | Dr. Max Weber.

The *Siboga* dredged stalked crinoids at seventeen stations, in all more than sixty specimens representing thirteen species and two additional varieties. Three of these species are referred to *Bathyerinus*, one to *Rhizocrinus*, two to *Isocrinus*, and the remainder to *Metacrinus*, while the species of *Rhizocrinus* dredged by the *Valdivia* off Somaliland and recorded by Chun in 1900 is included in the report, and figured under the name of *R. chuni*.

The species referred to the first two genera are of very exceptional interest, apart from the fact that neither genus has been recorded from the East Indian region; while the *Rhizocrinus* (*R. weberi* n. sp.) is related to *R. rawsonii* of the tropical Atlantic, the three *Bathyerinus* are in their characters quite unlike anything previously known; in the first place, they are all very small, one species, *B. poculum*, being only 8 mm. in total length, while none of the others exceeds 35 mm.; but, most remarkable of all, they unite the characters of *Bathyerinus* and *Rhizocrinus* so completely as to leave scarcely any grounds for considering them as distinct genera. This discovery was not news to the present reviewer; for the day after the receipt of Dr. Döderlein's work, his own description of two intermediate species, *Bathyerinus equatorialis* and *B. caribbeus* was published. *Rhizocrinus* (including, as we now apparently must, *Bathyerinus*) contains at the present writing fifteen described species, *of which nine have been made known during the past year, and there are several additional species now in press*; it is very evident that our knowledge of even this comparatively old genus is still extremely rudimentary. Dr. Döderlein's interesting remarks on the shedding of the arms in *Rhizocrinus*—*Bathyerinus* I shall consider in detail later.

Isocrinus naresianus, first found by the *Challenger*, was rediscovered by the *Siboga* off the northern end of Celebes, having been previously known only from the Kermadec and the Meangis Islands, and from Fiji, and a new species, *I. siboga*, was discovered near Timor. This last belongs to the group of the genus in which the costals and division series consist of two joints, bound by syzygy, including such species as *I. wyville-thomsoni*, *I. parrae* (of Guérin 1835 = *I. mülleri* of Oersted 1856 of which *I. maclearanus* is merely a variety) and *I. alternicirrus*, to the last of which *I. siboga* is most nearly related, though it possesses the normal arrangement of the cirri. The form *maclearanus*, by the way, did not come from the southwest Atlantic as stated by

Dr. Döderlein, but from the west central Atlantic; moreover, it was first described by Wyville Thomson, and not by Carpenter; also *I. wyville-thomsoni* was first described by Wyville-Thomson, Jeffries's mention of the name being in both cases a pure *nomen nudum*.

The discussion of *Metacrinus* is appropriately begun with an account of the infrabasals of *M. acutus*, which are compared to those of *Millericrinus polydactylus*. Then follow paragraphs on the specific characters of the genus found in the calyx, the arms, and the stem, which last is believed to furnish the most reliable characters. In this conclusion I heartily concur. The stems are considered at some length, and there is an interesting account of the stem growth, a subject which I shall discuss at some length later. Dr. Döderlein believes that, when living on the seabottom, the species of *Metaerinus* have very long stems, which are inextricably entangled one with another, forming a sort of mesh-work, from which the younger part of the stem and the crowns stand out; in other words, that the individuals form a sort of erinoid colony, the crowns arising from a maze of stems, and he adduces considerable strong evidence in support of this view. It will interest him to know that I have additional evidence pointing to the same conclusion.

The species of *Metaerinus* obtained by the *Siboga* all fall into that division of the genus in which there are "five radials," two of which are united by syzygy; three new species, *M. acutus*, *M. serratus* and *M. suluensis*, and a new variety, *M. nobilis timorensis*, are described, while that somewhat unhappy word *typica* is used to denote the typical forms of *M. nobilis* and *M. superbus*. *M. acutus* comes from the Ki Islands, and *M. serratus* and *M. suluensis* were found in the Sulu Archipelago. *M. nobilis timorensis*, as its name indicates, occurs near Timor. *M. cingulatus*, previously known from the Ki Islands and the Arafura Sea, was rediscovered at the Ki Islands and at Timor; *M. varians*, from the Kermadec and Meangis Islands, was found at Timor, the Ki Islands, and in the Sulu Archipelago; and a varietal (unnamed) form of *M. superbus* was collected at the Ki Islands. *Metacrinus nobilis* is divided into three varieties, *typica*, *murrayi* and *timorensis*, with somewhat unfortunate nomenclatorial results; for the specific name *murrayi* of Carpenter has precedence over *nobilis* of the same author. Expressing these names as trinomials, we have *Metacrinus mur-*

rayi murrayi, *M. murrayi typica*, and *M. murrayi timorensis*, the *typica* not representing the typical form at all. *Metacrinus murrayi* (to speak with nomenclatorial accuracy), previously known from the Ki Islands and Arafura Sea, was again found at the Ki Islands, and also at Timor.

Perhaps it is wisest to do as Dr. Döderlein has done and recognize by name the various geographical variations of the *Metacrinus* species, but it certainly conjures up a terrible vista of possibilities, for it is difficult to imagine more variable organisms than the species of this genus, according to my experience. I have examined some additional varieties of *M. superbus* from Japan, and a bewildering, though small, series of *M. angulatus* from the same locality, some of which fall into the group with "five radials," and others into that with eight to twelve, while *M. rotundus*, also falling into both groups, is even more variable; and it seems to me that if we once get a good start on the trinomial system in any branch of the recent crinoidea, with the continuance of the present activity in the field, it will not be long before each genus will require a specialist for its elucidation.

Dr. Döderlein is to be congratulated on the results of his study of the *Siboga* collection, and the production of a volume which will long stand as the authoritative work on the stalked crinoids of the East Indian seas, and which not only treats of the stalked crinoids systematically as a class, but suggests many interesting new lines of investigation, and bears throughout the stamp of one who not only has an exhaustive knowledge of the group under consideration, but of many different forms of animal life as well.

AUSTIN HOBART CLARK.

U. S. BUREAU OF FISHERIES.²

ANIMAL PATHOLOGY

Trypanosome Diseases.—Recent investigations which have been carried out in foreign laboratories with the object of ascertaining the mode of cure for sleeping sickness, and other trypanosome diseases have resulted in demonstrating certain features in the biological conduct of these protozoa towards chemical stimuli which are of extreme interest. Thus far only three

² Published with the permission of the Commissioner of Fish and Fisheries.

groups of chemicals have been discovered which are efficient in the treatment of trypanosome infections. They are: (a) benzidin dyes, (b) basic triphenyl-methane dyes and (c) arsenical compounds. In experimental animals complete cure has apparently been effected by maximum doses of these compounds. With lesser doses and prolonged treatment the parasites may disappear from the blood for a time, but later on make their appearance again. Those which recur have undergone a pronounced change in their biological characters and constitute a strain resistant to the therapeutic agent employed. Such a strain manifests chemo-resistance of a specific character towards the particular substance used to develop it and an increased resistance towards other compounds of the same group. On the other hand, the development of resistance towards one group causes no increase whatever in the resistance towards other groups. By continued experiments, however, a strain has been produced manifesting a triple resistance, specific towards each of substances employed.

Chemo-resistance, once acquired, persists unchanged while the resistant trypanosomes are passed through normal animals even for one hundred and forty transfers extending over fourteen months. This has been cited as strong evidence of the transmission of acquired characters. The specificity of the resistance is very striking. After an experimental animal has been inoculated with a mixture of two resistant strains and is then treated with a substance towards which one of the elements is resistant, the other element will disappear from the blood, but the resistant strain will remain and develop unchecked. Indeed, the two strains remain separate and capable of isolation after repeated passages through infected animals. Or, in other words, a strain with double resistance or with modified resistance does not arise as the result of infection with a mixture of two resistant strains.

HENRY B. WARD.

ANIMAL BEHAVIOR

Recent Work on the Behavior of Higher Animals.—There exist to-day two main centers for the strictly scientific and experimental study of the behavior of the higher animals. One is at Harvard, led by Yerkes, the other at Chicago, under Watson. Excellent work appears at times from other quarters, but it can usually be traced to the influence of one of the two men named. There is a third independent center for such work at Clark

University, but lacking the single-minded leadership of the other two, the attack on the problems has there been less unified and effective. Thorndike, whose work some years ago gave such impetus to the whole subject, has unfortunately been drawn into other work, or we should doubtless have another most effective center for such investigations. Outside of the United States the scientific study of the behavior of the higher animals is a negligible quantity, compared with what comes from the centers named. The active French movement in comparative psychology, under the influence of Bohn, Piéron and others, has been thus far limited mainly to the invertebrates.

At the laboratories we have named the work on animals is carried on by the aid of such accurate appliances and methods as have long been developed for the investigation of the physiological psychology of man, with ingenious modifications and additions as required by the peculiarities of the subject. This gives the work an almost uniquely precise and scientific character, as compared with most other studies in animal behavior. Most other workers have been compelled to content themselves with apparatus, observations and experiments of a more "home-made" character. The two leaders, one by training a zoologist and psychologist, the other a physiologist and psychologist, have been devoting themselves largely to rats and mice of late, while followers have made side excursions into the territory of cats, dogs and raccoons. It is necessary to concentrate the attack somewhere, and for the present the rats and mice are bearing the brunt. We shall pass in review the recent contributions from the centers named, limiting ourselves at present to work influenced from Harvard.

From Harvard we have first the elaborate study of the dancing mouse, by Dr. Yerkes.¹ Perhaps the most striking feature of this work lies in the elegant and fertile methods devised by the author, and of course in the results attained by these methods. The main method consists in a sort of "Lady or the Tiger" alternative presented to the unsuspecting mouse. He is invited to enter one of two doors: one leads to an electric shock, the other to freedom and food. The fateful portals are marked with signs of various sorts,—cards of different shapes, markings, color, brightness, odor, etc. The "right" and "wrong" doors can be alternated at the will of the experimenter, as can

¹ Yerkes, R. M. *The Dancing Mouse, a Study in Animal Behavior*. The Animal Behavior Series, Vol. I, 290 pages. The Macmillan Co., 1907.

the signs. The mouse in repeated experiments tries at first the simple plan of returning to the right or left door according as he has found that to be correct. When he finds that the correct portal is being alternated, he quickly learns to alternate in his choices. But when he finds that there is no regularity in the alternations, he begins to pay careful attention to the signs posted about the portal; "to run from one to the other, poking its head into each and peering about cautiously, touching the cardboards at the entrances, apparently smelling of them, and in every way attempting to determine which box could be entered safely." Often the mouse runs from one portal to the other twenty times or more, before deciding which to enter. Now, it is in this state of uncertainty and concern that the mouse is ready to give interesting results in animal education and in sense physiology. He uses all his senses to the best of his ability in determining which is the "right" door to enter, so there is opportunity, which Dr. Yerkes has skilfully used, to test his senses, and at the same time to study his ability to learn. When the two portals are indicated, the "right" one by a light card, the "wrong" one by a dark card, the mouse learns to choose the correct card. If for both cards are substituted others that are of deeper shade, but have a similar relative brightness, the mouse continues to choose the one of lighter shade. He has learned, not that a particular card, or a particular shade, is the right one, but that the *lighter of the two* is the one to choose; he often runs back and forth many times, seeming to compare them carefully. By accurately grading the difference in brightness between the two portals, it was possible to determine just what differences the mice could discriminate, giving an opportunity for work on Weber's law. *The mice rapidly learned to discriminate finer and finer shades of difference.* A certain mouse, in a first series of experiments, could discriminate only when the difference in brightness was practically half the greater brightness. In a later series he could discriminate when the difference was but one fifth and, after much more practise, when the difference was only one tenth. Such educability at first carried confusion into the data designed to test Weber's law. When it was finally taken into account, the law was found to hold.

In a similar way Yerkes made extensive studies of color vision in the mouse. He found that apparently they do not see colors,

at least not as we do; that most of their apparent discrimination of color is due to differences in brightness; and that the brightness of different colors is not the same for them as for ourselves.

It is extraordinary that the mice were unable to discriminate the portals by different shapes of cards or of lights. They showed no power of distinguishing forms.

We have given some samples of Yerkes' methods and results; many other matters of equal or greater interest were studied, by varied methods, including the classic one of using labyrinths. The author's experiences are set forth in interesting chapters on educability, methods of learning, the efficiency of different methods of training, the duration of habits, the revival of lost habits, individual differences in behavior, and the like. When it comes to responding to experimentation, the dancing mouse is, as its name indicates, rather an artistic than a strictly utilitarian animal, giving a delightful variation from those orthodox creatures whose main desire is to "get there," so that results are not readily expressed correctly in terms of minutes required and space passed over. "Most mammals which have been experimentally studied have proved their eagerness and ability to learn the shortest, quickest, and simplest route to food without the additional spur of punishment for wandering. With the dancer it is different. It is content to be moving; whether the movement carries it directly to the food-box is of secondary importance. On its way to the food-box, no matter whether the box be slightly or strikingly different from its companion box, the dancer may go by way of the wrong box, may take a few turns, cut some figure-eights, or even spin like a top for a few seconds almost within vibrissa-reach of the food-box, and all this, even though it be very hungry."

In addition to the strictly experimental work, Yerkes gives a full account of the peculiar "dancing" movements that have given the animal its name; a sketch of what we know of its history, and an extensive discussion of the disputed question as to whether its ears are defective and whether it is deaf. Yerkes concludes that it can hear only for a few days, when about two weeks old.

Altogether, Dr. Yerkes' book is one of the most attractive as well as one of the most valuable of the strictly scientific studies of animal behavior. It would be venturing out of the

"strictly scientific," but one wishes that the author might give us an imaginative picture of what life and the universe may be in the consciousness of this little creature, that does not hear, sees little or nothing of colors, can't distinguish a square box from a round one nor a circular card from a triangular one, feels impelled to "cut figure eights and spin like a top" on its way to a dish of food, and learns many things rapidly and well. Possibly such an unscientific picture could be appended to the really scientific account without injury to the latter! Dr. Yerkes is still studying the dancing mouse, and may some time feel prepared to give us such a picture.

Or perhaps we must look for such pictures to the second volume of the Animal Behavior Series, of which this is the first! The second one, just announced, is a volume on "The Animal Mind," by Margaret Washburn. The Series, edited by Dr. Yerkes, promises to be of the greatest value.

A matter that has been most in need of study is the part played by imitation in the behavior of higher animals. Years ago imitation was the favorite refuge of those who wished to explain the remarkable actions of animals without attributing to them higher intellectual powers. When Tabby pressed the latch and walked out the door, that was because she had seen some one do it. Then came Thorndike, and changed all that. To give imitation in place of reason as an explanation, says Thorndike, is to substitute one false explanation for another. In studying cats and monkeys, Thorndike saw no signs of imitation either of one another or of man. And most later investigators have agreed that imitation plays little part in the behavior of animals, at least in comparison with what had been supposed. Even the monkey, we are told, rarely imitates man or other monkeys. Direct, unreflective imitation of simple sounds or movements—the performance of an act merely because a companion has performed it, without reference to results—is less rare, though likewise not so common, as had been supposed. But the imitation of an act because that act accomplished a certain result, and in order to accomplish the same result—this was not found, though this is the kind of imitation assumed in current explanations to be common. The extensive experiments of Hobhouse,² evidently undertaken with the expectation of finding imitation playing a part, are striking as an example of how

² *Mind in Evolution*, Chapter VIII.

little it is possible to find, and how uncertain is what is found, even with the best of will. Kinnaman, Small and others had incidentally seen a few examples of real imitation. We have now from the Harvard laboratory two careful studies of this matter by Berry,³ with results that are most interesting. In Berry's rats and cats we find imitation as it were in the making. Our conception of imitation, and of its different kinds, loses its sharp lines and angles and becomes indefinite. When one knows how to escape or get food and another does not, the animals do not set to work to imitate each other's actions in the clear-cut way we are apt to think of as imitation. But the one that doesn't "know how" does after some time begin to pay attention to his comrade's actions, and then in an indefinite way to do something of the same sort himself. "We found that when two rats were put into the box together, one rat being trained to get out of the box, and the other untrained, at first they were indifferent to each other's presence, but as the untrained rat observed that the other was able to get out, while he was not, a gradual change took place. The untrained rat began to watch the other's movements closely; he followed him all about the cage, standing up on his hind legs beside him at the string, and pulling it after he had pulled it, etc. We also saw that when he was put back the immediate vicinity of the loop was the point of greatest interest for him, and that he tried to get out by working at the spot where he had seen the trained rat try."⁴ In cats similar and more marked cases of imitation were found and analyzed.

Berry's work is the first really scientific study of imitation in animals that we have had, and it shows, as so commonly happens when a thorough study is made, that we can not make extreme statements, whether positive or negative. Imitation is found; even "reflective imitation," but it is not precise; we can often hardly be certain whether it *is* imitation; and where it is more pronounced it is difficult to distinguish imitation for the mere sake of doing what a companion does, from imitation for the purpose of accomplishing the result that the companion accomplishes. Like all other traits of behavior, imitation grows gradually out of something that seems not the same thing at

³ Berry, C. S. The Imitative Tendency of White Rats. *Journ. Comp. Neurol. and Psychol.*, 16, 333-361. *Id.*, An Experimental Study of Imitation in Cats. *Ibid.*, 18, 1908, pp. 1-25.

⁴ Berry, The Imitative Tendency of White Rats, p. 358.

all! Whether to call this "something" by the same name as the developed activity is one of the frequent grounds for unprofitable controversy.

L. W. Cole⁵ has made an elaborate investigation of the intelligence of raccoons, with results of more than usual interest. The raccoons are compared throughout with the famous cats of Thorndike, and the work, like most recent work on animal intelligence, follows the outlines of the well known paper of the author just mentioned.⁶ But Cole has made real and important advances in both method and results. The raccoons are either much more clever than the cats, or the methods employed were better fitted for bringing out latent possibilities; probably both these things are true. The experiments consisted largely in allowing the animals to learn to open boxes closed by fastenings of various degrees of complication. The raccoons learned somewhat more readily than the cats. As in all other animals, their learning was largely by trial and error. But they are not restricted exclusively to that method, as Thorndike maintained to be the case for the cats; decidedly not if we limit that method to the *gradual* formation of an association between a motor impulse and a sense perception. (1) There was clear evidence that the animal at times, catches the idea, that a certain act is what opens the door, so that he later acts directly and at once on that idea. It is not a mere gradual exclusion of useless movements, till only the useful ones are left. (2) The raccoon learns by being put through an act. It learns without "innervating its muscles," the great test for the possibility of learning in Thorndike's cats. It learns to go into a box by a certain entrance, through having been *lifted* into the box that way a number of times. By being put through them, it learns certain acts which it was unable to learn by its own efforts. By putting different raccoons through the same act in different ways, they learned to perform it in different ways; for example, one learned to lift a latch with its paws, another with its nose. (3) While the raccoons, like most other animals, do not imitate each other or any one else in a marked degree, they did, after seeing the experimenter perform a certain action many times, "catch the idea" and endeavor to perform the action for themselves. This

⁵ Cole, L. W. Concerning the Intelligence of Raccoons. *Journ. Comp. Neurol. and Psychol.*, 17, 1907, pp. 211-261.

⁶ Thorndike, E. L. Animal Intelligence. *Psychol. Review, Monograph Suppl.*, vol. 2, 1898.

is of course the essence of "reflective" imitation. (4) Thorndike concluded that cats have probably no "free ideas"; no stock of images which are motives for acts. The association in the cats was always between a motor impulse and a present sense perception; there was no association of *ideas*. This negative conclusion was based largely on the inability of the animals to learn from being put through an act. In this latter matter, Cole calls the reader's attention particularly to "*the radical difference at every point*" between the cats of Thorndike's experiments and the raccoons of his own. "If inability thus to learn is evidence against the presence of ideas, then ability to do so should be equally strong evidence for it." Furthermore, Cole gives much additional evidence for the presence of ideas in the raccoons; and certain results of some extremely ingenious experiments amount to a demonstration that the animals do hold mental images, so far as such a thing can be demonstrated.⁷ The animals seemed to remember definite objects for a time, then forget them; then suddenly, under certain conditions, recall them. They fought against being put into boxes with complex fastenings, from which they had some time before had difficulty in escaping, though they willingly went into similar boxes whose fastenings they had found simple. In certain experiments there were two alternative signs to be raised; the green one meant food, the red one meant none. The raccoons learned to raise these signs by clawing at the standards, but they could not see beforehand which sign would come up by clawing at a certain standard. When the red one came up they clawed it down again, then clawed up the green one, and made ready to receive food. Clearly, the red sign did not correspond to an image that the animal had in mind, while the green one did. Other experiments were devised in which success depended on the animal's holding in mind the images of certain things that had gone before; the raccoons stood these tests successfully. It is difficult to see how there could be more conclusive proof of the presence of ideas in animals that can not talk.

⁷ A subjective thing, such as an idea, can, of course, not be absolutely demonstrated by objective methods. It is always possible to substitute for the idea its physiological accompaniment, and say that this is all that we can be assured of. In other words, "demonstration of the existence of ideas" in animals can never go further than to show that *they act as men do when men have ideas*.

The interesting paper of Hamilton⁸ is perhaps in its origin independent of the Harvard laboratory. We have seen that the dancing mouse learns to act on the basis of a comparison between two things, selecting, not a particular thing, but the lighter of two, or the darker of two, etc. Kinnaman found that the monkey could similarly learn to choose always the lighter vessel, or to choose the colored vessel from among a number of vessels, even when the colors were changed. Hamilton made a precise study of a similar sort of action in a dog. The animal learned that in order to escape from a pen and get food he must press, out of a number of levers, the one that bore the same sign that was found on a general sign-board elsewhere in the pen. In successful cases his method of procedure was, then, to inspect the general signboard, then to pass in review the four levers till he found the one that bore the same sign—then to press this. This appears to involve a fairly complex mental operation (if we may venture to interpret the actions of animals from that highly reprehensible standpoint). The dog clearly learned to choose in the way described. But unfortunately, being a clever dog, he after a time discovered a much simpler method of action that accomplished the same results. He merely began at one end of the series and pressed the levers in order till he came to the one that worked. When electric shocks were attached to the "wrong" levers, he decided that he didn't care to play at that game any longer, and the experiments had to end.

How far such action, seeming to involve complex mental operations, may be demonstrated in animals when there has been fifty years' development of method and results in such investigations, instead of merely two or three attempts at it, is a question that deserves consideration by those who are so ready to deny, on the basis of what we now know (or rather, on the basis of what we don't know), all mental complexity in animals.

Indeed, it is clear that much of the work we have just reviewed consists in showing experimentally that the mental operations of animals are more complex than had been supposed; in restoring to animals certain things that had been denied them. And this is typical. The recent history of the study of animal

* Hamilton, G. van T. An Experimental Study of an Unusual Type of Reaction in a Dog. *Journ. Comp. Neurol. and Psychol.*, 17, 1907, pp. 329-341.

behavior has shown a curious parallelism in each of its three great divisions. In each division the slate was, as it were, wiped clean some ten or fifteen years ago; the existing structure was razed to the ground, and we have been building it up again ever since. In the lower organisms Loeb reduced the phenomena to almost inorganic simplicity. For the ants, bees and other higher invertebrates Bethe took similar action; they were stripped of their fanciful decorations of memory, intelligence, etc., and left absolutely devoid of "psychic qualities" of any sort; their behavior was composed of invariable reflexes and tropisms of the simplest character. Thorndike performed the same operation for the vertebrates. Not only did they not reason (preposterous notion!), but they did not imitate, could not learn by seeing a thing done nor by being put through an act, nor by any other way than by simply gradually dropping out useless movements from among those made at random; and they had not even *ideas* of things past, to say nothing of perceiving relations or being capable of trains of thought or of formulating a plan.

In all three divisions of the subject the work since these operations has consisted largely in the slow and painful restoration, by precise experimental methods, of what was thus wiped out at one fell swoop. The three authors named, with those that aided them, perhaps did the science of behavior the greatest possible service at that time. Before them there was hardly an ordered science in this subject; there was a jungle of suppositions, assumptions and anecdotes. Loeb, Bethe, Thorndike and Company destroyed all this and compelled us to rebuild from the ground up, a solid structure, based on precise scientific methods. How high the structure will have to go, no one can foretell; certainly it is not yet finished. Indeed, animal behavior as a science is merely in its swaddling clothes; it can not carry as yet many sweeping conclusions, particularly negative ones. General negations based on what we now know are most unscientific; they are largely capitalizations of our large stock of ignorance. It behooves the man of science, therefore, to be careful in his destructive criticisms; some recent controversies show that this caution is much needed. It will be long before our science is coextensive with the phenomena with which it is attempting to deal.

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